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SCIENCE

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Volume 127, Number 3291

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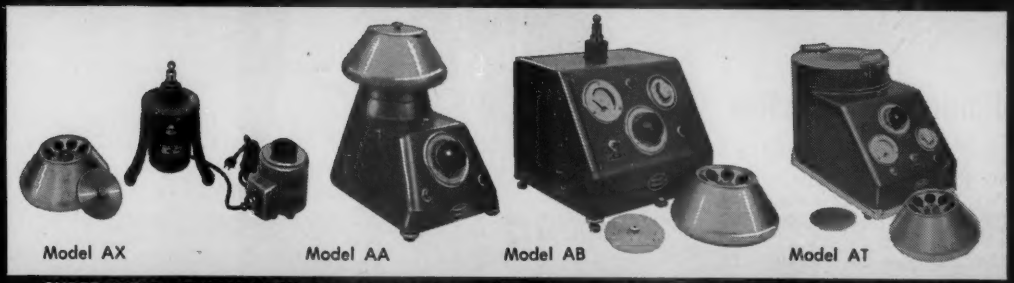

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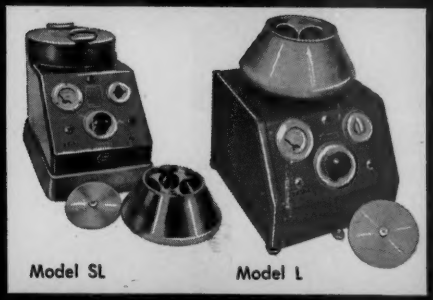

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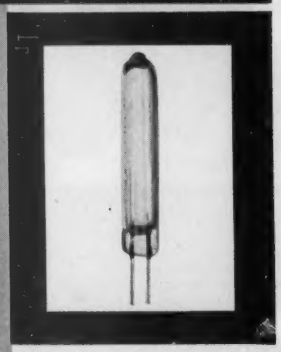

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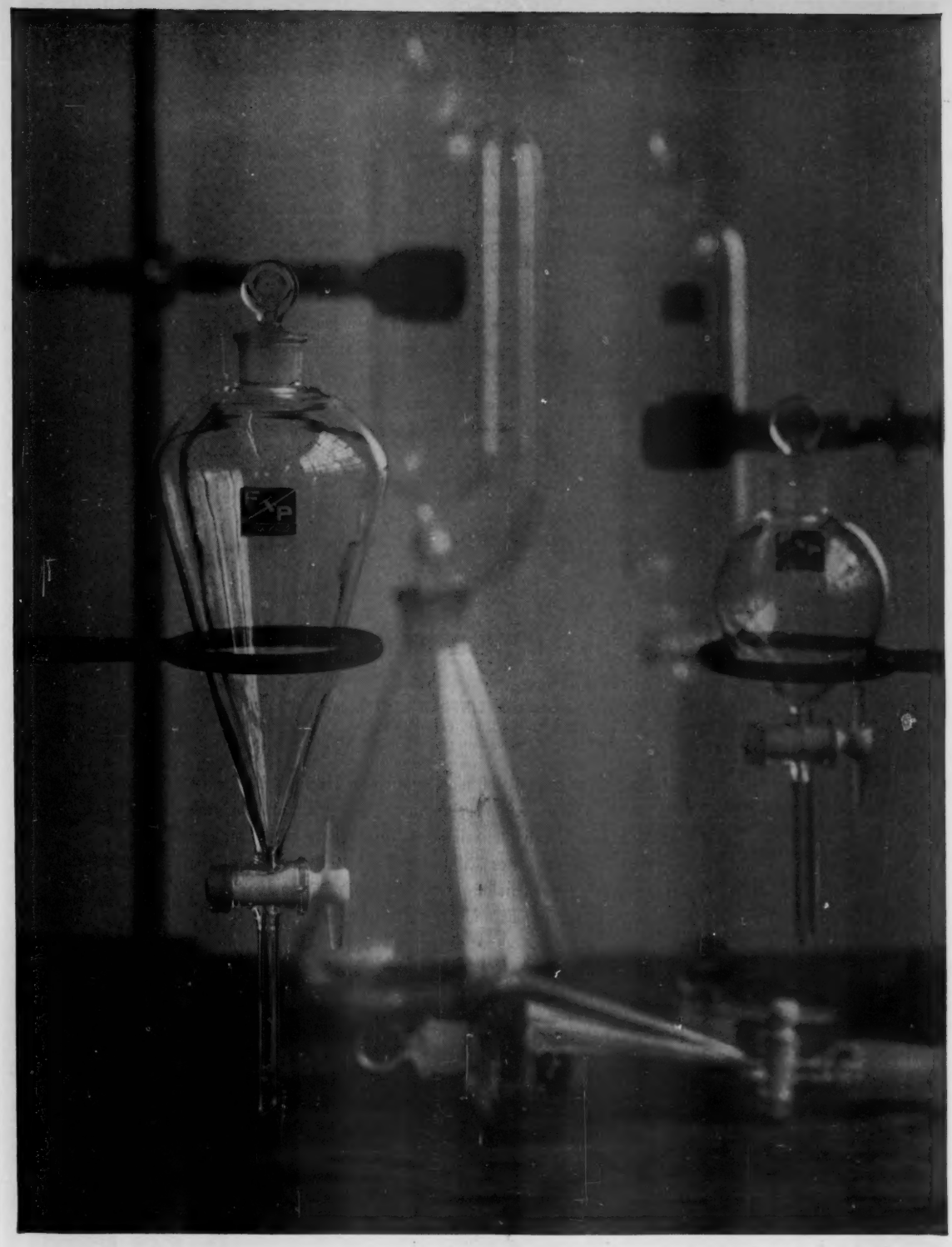

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SCIENCE, founded in 1880, is published each Friday by the American Association for the Advancement of Science at Business Press, Lancaster, Pa. Entered at the Lancaster, Pa., Post Office as second class matter under the Act of 3 March 1879.

SCIENCE is indexed in the *Reader's Guide to Periodical Literature*.

Editorial and personnel-placement correspondence should be addressed to SCIENCE, 1515 Massachusetts Ave., NW, Washington 5, D.C. Manuscripts should be typed with double spacing and submitted in duplicate. The AAAS assumes no responsibility for the safety of manuscripts or for the opinions expressed by contributors. For detailed suggestions on the preparation of manuscripts, book reviews, and illustrations, see *Science* 125, 16 (4 Jan. 1957).

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Full Circle

With the recent appointment of Wallace R. Brode, associate director of the National Bureau of Standards and president of the American Association for the Advancement of Science, as its science adviser, the Department of State has taken the first step toward the resuscitation of a program that has been sadly neglected in recent years.

The program now being revived was an outgrowth of an experimental use of scientific attachés at the London embassy in 1947–49. Experience with this arrangement was promising enough to lead the Secretary of State to explore the possibilities of an expanded program. To this end he appointed Lloyd V. Berkner, at that time on the staff of the Carnegie Institution of Washington, to study the problems which scientific and technological developments posed for foreign policy.

The ensuing report [*Science and Foreign Relations*, Department of State Publication 3860, General Foreign Policy Series 33 (May 1950)] recommended the establishment of the Office of Science Adviser to the department and the appointment of science attachés at several European embassies. These recommendations were followed, and the first appointments were made in 1951; by 1952 ten attachés were on duty. But, for reasons that have never been made public and despite approval of the work by Congress, the department allowed the program to decline by the simple expedient of not refilling the posts when they became vacant: there has been no science adviser since 1953, and the last attaché returned more than two years ago. The only continuity the program has had has been provided by the assistant to the science adviser, Walter Rudolph, an "economist who had had much to do with organizing the original plan, and who . . . has done a herculean job in holding the few remaining pieces together" [Lloyd V. Berkner, *Foreign Affairs* **36**, 229 (Jan. 1958)].

The decline of the program aroused concern among scientists. Thus, Dael Wolfle commented, "By failing to appoint a science adviser and science attachés the Department of State is depriving itself—and the country it represents—of one of its authorized assets" [*Science* **123**, 205 (10 Feb. 1956)]. More recently W. H. Forbes, in considering the role of scientists in the department, wrote, "The subjects with which they deal have long had great economic impact and have now a rapidly increasing significance in matters of government and international relations. . . . This presents opportunities for international cooperation which should be utilized. . . ." [*Bulletin of the Atomic Scientists* **8**, 275 (Oct. 1957)].

A similar concern has also found expression in Congress, especially in the House Committee on Government Operations. In 1956 the committee by unanimous vote urged the State Department to restore the advisory program.

Whether the decision to restore the program was a belated response to these urgings or a fairly prompt response to more recent events is not now of major importance. What is important is that matters have come full circle and that the department is reestablishing a system that will again give it the benefit of scientific advice on scientific questions that may affect foreign policy and foreign relations.—G. DuS.

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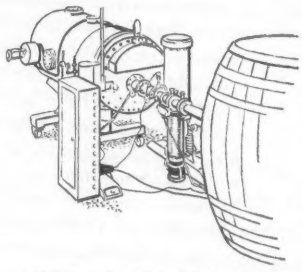
Borst[2], using a 2-Mev Van de Graaff with the Be^{9} (d, n) reaction, obtained an output of about 5×10^{10} neutrons per second and a neutron multiplication of 6.7 in a water-moderated "pickle barrel". The assembly had a criticality factor of 0.85. With the high neutron output from the Van de Graaff, it is possible to carry out lifetime studies of prompt and delayed neutrons, measurements of the effect of impurity in the water moderator and the effect of control rods on the criticality factor, measurements of cross-sections, and many other experiments concerned with neutron physics.



Van de Graaff pulsed-neutron source with subcritical assembly in pickle barrel.

Sjorstrand[3] of Sweden has reported measurements on a subcritical heavy-water reactor using pulsed neutrons. By this method it was possible to determine the criticality per unit volume of moderator and the effect of control-rod geometry on criticality. The overall criticality of the reactor could be measured each time some operating parameter was changed.

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Reference
1. Campbell, P. H. Stelson Bull. APS 1, No. 4, 183, 1956
2. L. B. Borst — Nucleonics Aug. 1956. Vol. 14, No. 8, Pg. 66-68
3. N. G. Sjorstrand — Vol. 5, 32, National Conference on Peaceful Uses of Atomic Energy — Geneva, 1955 — P/789

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Inductive Inference: A New Approach

Israel Scheffler

On what grounds do we choose the theories by which we anticipate the future? How do we decide what to predict about cases never before observed? These questions concerning what is traditionally called "induction" are among the most fundamental and most difficult which can be asked about the logic of science. Much reflection has been devoted to these questions in recent years, but no contribution has proved more incisive and challenging than that of Nelson Goodman of the University of Pennsylvania, whose papers on induction and allied problems have activated lively philosophic controversy over the past twelve years.

In 1955, Goodman published *Fact, Fiction, and Forecast* (*1*), in which he presented the outlines of a new approach to the understanding of induction. This recent work has also aroused considerable comment by philosophers, both in print and out, and it is safe to say that the discussion is still in its early stages. The scientific public is, however, largely unaware of this new development, just as it was largely unacquainted with the controversies that preceded it. If there is no real boundary between science and the philosophy of science, the consideration of fundamental research in the logic of science ought not to be confined, even at the early stages, to circles of philosophers. The aim of this article is thus to acquaint the scientific reader with the background and the direction of Goodman's investigations, as they bear on the interpretation of induction.

The author is lecturer on education in the Graduate School of Education, Harvard University, Cambridge, Mass.

Hume's Challenge and the Generalization Formula

The starting point for all modern thinking about induction is David Hume's denial of necessary connections of matters of fact: between observed cases recorded in the evidence and predicted cases based on the evidence there is a fundamental logical gap, which cannot be bridged by deductive inference. If, then, the truth of our predictions is not guaranteed by logical deduction from available evidence, what can be their rational justification? This challenge, arising out of Hume's analysis, has evoked a variety of replies. Leaving aside the reply of the skeptics, who are willing to admit that all induction is indeed without rational foundation, and that of the deductivists, who strive vainly to show Hume wrong, we find two replies which have gained wide popularity, the first primarily among philosophers, the second among scientists as well.

The first reply criticizes the assumption that rational justification can be only a matter of deduction from the evidence, pointing out that the normal use of expressions such as "rational," "reasonable," "based on good reasons," and so forth sanctions their application to statements referring to unexamined cases, and hence not deducible from accumulated evidence. This reply, although true, is, however, woefully inadequate. For not every statement which outstrips available evidence is reasonable, though some are. Outstripping the evidence is, to be sure, no bar to rationality, but neither does it guarantee rationality. If we are to meet the challenge posed, we must go on to formulate the specific criteria by which some inductions are justified as reasonable while others are rejected as unreasonable, though both groups outstrip the available evidence. Now it is likely that at least part of the reason why this further task has been slighted is that the adequacy of the second reply has largely been taken for granted.

This second reply, stated in one form by Hume himself, is that reasonable inductions are those which conform to past regularities. In modern dress, it appears as the popular assertion that predictions are made in accordance with general theories which have worked in the past. What leads us to make one particular prediction rather than its opposite is not its deducibility from evidence but rather its congruence with a generalization thoroughly in accord with all such evidence, and the correlative disconfirmation of the contrary generalization by the same evidence. (I shall refer to this hereafter as the "generalization formula"). Of course, if no relevant evidence is available to decide between a given generalization and its contrary, or if the available evidence is mixed, neither generalization will support a particular inductive conclusion. But it is only to be expected that every limited body of evidence will fail to decide between *some* generalization and its contrary, and hence that we will generally not be able to choose between *every* particular prediction and its opposite. It is sufficient, therefore, for a formulation of the criteria of induction to show how certain bodies of evidence enable us to decide between certain conflicting inductions. This the generalization formula seems to accomplish. For if there is evidence which consistently supports a given generalization, then the contrary generalization is *ipso facto* disconfirmed, and our particular inductive conclusions seem automatically selected for us. There are,

of course, details to be taken care of, relating to such matters as the calculation of degrees of support which generalizations derive from past evidence, but, in principle, we have our answer to the challenge of induction.

Goodman's Refutation of the Generalization Formula

It is this sanguine estimate which has been thoroughly upset by Goodman's researches. Published in 1946 and 1947, his early papers in the philosophical journals dealt with a variety of interrelated questions: the nature of scientific law, of dispositional properties, of potentiality, of relevant conditions, of counterfactual judgments, of confirmation or induction (*2*). They immediately aroused a storm of controversy. What made the papers so disturbing to the philosophic community was the fact that, while all these questions were shown to be intimately connected, Goodman's logically rigorous attempts to answer them without going around in circles ended in a big question mark. Appearing at a time when logicians had been making considerable progress in analyzing other aspects of scientific method, these results came as a shock. Goodman's investigations, it seemed, had sufficed to undermine all the usual formulas concerning the most basic concepts of the logic of science, but his repeated and ingenious efforts to supply a positive alternative had all turned out fruitless. In the philosophic discussions that followed, every attempt was made to skirt Goodman's disheartening results. They were declared unimportant for the practicing scientist. The initial questions were asserted to be insoluble, hence worthless. Many papers, on the other hand, proposed what seemed perfectly obvious solutions that turned out to be question-begging. Only a very few authors fully recognized the seriousness of the situation for the philosophy of science and tried to cope with it directly (*3*).

In 1953, with the whole matter still very much unsettled, Goodman delivered a series of three lectures at the University of London, in which he again addressed himself to the problem. These lectures, together with his major 1946 paper, were then published together in his book *Fact, Fiction and Forecast,* which appeared in 1955 (*1*). Here Goodman essayed a new and positive approach to some of the major questions he had faced earlier. He did not offer his book as a final solution to all the original problems. He did, however, present a fresh approach, worked out with sufficient rigor to put discussion of it on a fruitful basis. But we are getting ahead of our story and must now return to see how Goodman's early work affected the theory of induction.

How did Goodman's early papers upset complacency with respect to the generalization formula (according to which we make these predictions congruent with generalizations thoroughly in accord with past evidence)? We may profitably approach this matter in the light of a passage from J. S. Mill's *Logic.* Although it does seem true that, for every particular induction we make, there is some generalization related to it in the manner described, Mill argues that generalizations which are equally well supported by available evidence vary in the sanction they provide for their respective particular inductions: "Again, there are cases in which we reckon with the most unfailing confidence upon uniformity, and other cases in which we do not count upon it at all. In some we feel complete assurance that the future will resemble the past, the unknown be precisely similar to the known. In others, however invariable may be the result obtained from the instances which have been observed, we draw from them no more than a very feeble presumption that the like result will hold in all other cases. . . . When a chemist announces the existence and properties of a newly discovered substance, if we confide in his accuracy, we feel assured that the conclusions he has arrived at will hold universally, though the induction be founded but on a single instance. . . . Now mark another case, and contrast it with this. Not all the instances which have been observed since the beginning of the world in support of the general proposition that all crows are black would be deemed a sufficient presumption of the truth of the proposition, to outweigh the testimony of one unexceptionable witness who should affirm that in some region of the earth not fully explored he had caught and examined a crow, and had found it to be grey. Why is a single instance, in some cases, sufficient for a complete induction, while in others myriads of concurring instances, without a single exception known or presumed, go such a very little way towards establishing an universal proposition?" (*4*).

And Goodman gives an analogous example when he writes: "That a given piece of copper conducts electricity increases the credibility of statements asserting that other pieces of copper conduct electricity, and thus confirms the hypothesis that all copper conducts electricity. But the fact that a given man now in this room is a third son does not increase the credibility of statements asserting that other men now in this room are third sons, and so does not confirm the hypothesis that all men now in this room are third sons. Yet in both cases our hypothesis is a generalization of the evidence statement. The difference is that in the former case the hypothesis is a *lawlike* statement; while in the latter case, the hypothesis is a merely contingent or accidental generality. Only a statement that is *lawlike*—regardless of its truth or falsity or its scientific importance—is capable of receiving confirmation from an instance of it; accidental statements are not." (*1*, p. 73)

But it is Goodman's further formulation of the problem that is crucial. For what has so far been shown is that, in addition to all credible particular inductions, generalization from the evidence also would select certain incredible ones. Now Goodman shows that among these incredible ones lie the very negations of our credible predictions concerning new cases. To apply his previous example, it is not merely that by generalization we selectively establish, in addition to the credible prediction that the next specimen of copper will conduct electricity, also the incredible one that the next present occupant of this room to be examined is a third son. Rather, we do not even establish that the next specimen of copper conducts electricity, for we can produce a generalization equally supported by the evidence and yielding the prediction that it does not. Or, putting this point in the form of a specific example, while the available evidence clearly supports:

(S_1) All specimens of copper conduct electricity.

and clearly disconfirms its contrary:

(S_2) All specimens of copper do not conduct electricity.

this is not sufficient to yield the particular induction concerning a new copper specimen c, to be examined:

(S_3) c conducts electricity.

since the same evidence also and equally supports:

(S_4) All specimens of copper are either such that they have been examined prior to t and conduct electricity or have not been examined prior to t and do not conduct electricity.

while clearly disconfirming *its* contrary:

(S_5) All specimens of copper are either such that they have been examined prior to t and do not conduct electricity or have not been examined prior to t and do conduct electricity.

thus giving rise to the negate of S_3:

(S_6) c does not conduct electricity.

if it is assumed true that:

(S_7) c has not been examined prior to t.

For cases assumed new, then, the generalization formula selects no particular inductions at all. Merely to be told to choose our inductions by reference to theories which work relative to past evidence is hence to be given worthless advice. Nor does this situation improve with the accumulation of relevant data over time. For even if we later find S_6 false and add S_3 to our evidence, leading to a rejection of S_4, we do not thereby eliminate other hypotheses which are exactly like S_4 but which specify times later than t. Accordingly, no matter how much empirical data we have accumulated and no matter how many hypotheses like S_4 we have disconfirmed up to a given point in time, we still have (by the generalization formula) contradictory predictions for every case not yet included in our data. No matter how fast and how long we run, we find we are standing still at the starting line.

This predicament holds, of course, only for cases assumed to be new. Using our previous example, if neither S_7 nor its negate is assumed, then S_4 yields neither S_3 nor S_6, while if S_7 is assumed false, then S_4 coincides with S_1, implying S_3 rather than S_6. This is not surprising, however, since, if S_7 is false, c is identical with one of our original evidence cases, all of which are described by the evidence itself as conducting electricity; S_3 is thus implied deductively by the evidence at hand, given the general understanding that no cases have been omitted.

As soon as we leave the safe territory of examined cases, however, and try to deal with a new one, generalization yields contradictory inductions, deciding for neither. And, further, since the adoption of a generalization constitutes wholesale endorsement of appropriate particular inductions yet to be made, then even if we do not know about some specific case that it is a new one, our unrestricted adoption of generalizations gets us into trouble if we can make the assumption of novelty for at least one case within the appropriate range. Since, moreover, we patently do choose between contradictory inductions covering new cases, as well as between competing generalizations, the generalization formula must be wrong as a definition of our inductive choices. In our previous example, we obviously in practice would *not* hold S_4 equally supported by uniformly positive evidence supporting S_1, nor would we under such conditions have any hesitation in rejecting S_6 in favor of S_3. This clearly indicates that the generalization formula is not adequate to characterize our inductive behavior. We apparently employ additional, nonsyntactic criteria governing the extension of characteristics of our evidence-cases to other cases in induction.

These criteria of what Goodman calls "projectibility" select just those generalizations *capable* of receiving support from their positive instances and in turn sanctioning particular inductions. Projectible hypotheses may, in individual cases, fail to sanction any particular inductions (for example, in cases where we have two such hypotheses which conflict), but no non-projectible hypothesis sanctions any induction, no matter how much positive support it has in the sense of the generalization formula. Goodman's problem is then to define projectibility, which is, in turn, needed to define induction. Since counterfactual judgments (for example, "If this salt, which has not in fact been put in water, had been put in water, it would have dissolved.") are, moreover, construable as resting upon just such generalizations as are projectible, that is, legitimately used for induction (in this case, "Every sample of salt, when put into water, dissolves."), and, furthermore, are themselves used to explain dispositional predicates, such as "is soluble," the definition of projectibility would throw light on these additional issues as well.

Attempts to Repair the Generalization Formula

It may be thought that the characterization of projectibility can be accomplished rather easily, simply by ruling out generalizations making reference to time. Recall that, in our above example, the trouble arose because the available evidence equally supported S_1 and S_4. But whereas the predicate "conducts electricity" makes no reference to time, the predicate "has been examined prior to t and conducts electricity or has not been examined prior to t and does not conduct electricity" makes reference to time of examination, and moreover can be explained, given such reference, in terms of the former predicate. It may further be pointed out that, without assumption S_7 (making reference to time of examination), no contradiction arises. It is only when we add S_7 to S_4 that S_6, which contradicts S_3, is derived. Why not use this, then, as a rule for eliminating S_4—namely, its requiring an additional assumption about time of examination to produce one of our contradictory inductions?

The answer is that the situation is easily reversed. Symbolize the predicate "conducts electricity" by C and the other, more complicated one, of S_4, by K; symbolize "has been examined before t" by E. It is true that, as the present argument maintains, K is then definable as

(E and C) or (not-E and not-C)

("has been examined before t and conducts electricity or has not been examined before t and does not conduct electricity"). However, it is also true that, taking K as our primitive idea, C is definable as

(E and K) or (not-E and not-K)

Furthermore, in the latter mode of description, S_1 would become:

(S_1') All specimens of copper are either such that they have been examined prior to t and have the property K or have not been examined before t and do not have the property K.

while S_4 would become:

(S_4') All specimens of copper have the property K.

To derive a parallel to S_3, we need to show that a new case c does not have the property K. This we can do if we now supplement S_1' with S_7 getting:

(S_3') c does not have the property K.

And we derive our contradictory particular induction, parallel to S_6, from S_4', without using S_7:

(S_6') c has the property K.

Thus, neither the employment by a hypothesis of a predicate referring to time nor its need of supplementation by S_7 in order to produce contradiction is a reliable clue with which to try to repair the generalization formula. Neither is, strictly speaking, any clue at all.

But perhaps the generalization formula

is being applied too narrowly. We have, after all, been considering isolated statements in abstraction from other, relevant and well-established, hypotheses. In the above illustration we have, for instance, so far ignored the fact that available evidence also supports (by the generalization formula) a number of hypotheses of the following kind:

(S_8) All specimens of iron conduct electricity.

(S_9) All specimens of wood fail to conduct electricity.

and that these in turn lend credence to the following larger generalization:

(S_{10}) All classes of specimens of the same material are uniform with respect to electrical conductivity.

This larger generalization, having independent warrant and conflicting with S_4, serves thereby to discredit it, thus eliminating the troublesome induction S_6. In this way, it may be argued, the generalization formula can be rendered viable simply by taking account of a wider context of relevant hypotheses.

It takes but a moment of reflection, however, to see the weakness of such an argument. For, by reasoning analogous to that initially employed in introducing S_4, it will be seen that the very same evidence which supports S_8, S_9, and S_{10} also and equally (by the generalization formula itself) supports:

(S_8') All specimens of iron have the property K.

(S_9') All specimens of wood fail to have the property K.

(S_{10}') All classes of specimens of the same material are uniform with respect to possession of the property K.

This latter large generalization, it will be noted, produces just the opposite effect from that of S_{10}. It conflicts with S_1, thereby, by analogous argument, discrediting it and eliminating the induction S_3 rather than S_6. Which of these conflicting large generalizations shall we now choose to take account of, S_{10} or S_{10}'? It is evident that we are again face to face with the very problem with which we started and that the proposal to repair the generalization formula by referring to other relevant hypotheses selected by it serves merely to postpone our perplexity. For these other hypotheses, in conflict themselves, are of no help unless we have some way of deciding which of them are projectible. In the face of difficulties such as these, *it becomes impossible to explain our choice of predictions by reference to whether or not they accord with generalizations which work*, no matter how widely the scope of this principle is construed.

Goodman's New Approach

Goodman's new idea is to utilize pragmatic or historical information that may fairly be assumed available at the time of induction, and to define projectibility in terms of such extrasyntactic information. The generalization formula, it will be recalled, rests on the notion of an *accordance* between a predictive generalization and the evidence by which it is supported, an accordance which can be determined solely by an examination of the generalization and its evidence-statements. In this sense, the relation of accordance is formal or syntactic (as the relation of deduction is), making use of no material or historical information. Goodman now suggests that, in order to specify the predictive generalizations we choose on the basis of given evidence, we need not restrict ourselves merely to the syntactic features of the statements before us. Rather, he makes the radical proposal that we use also the historical record of past predictions, and in particular, the *biographies* of the specific terms or predicates employed in previous inductions. Our theories, he suggests, are chosen not merely by virtue of the way they encompass the evidence, but also by virtue of the way the language in which they are couched accords with past linguistic practice.

His basic concept is "entrenchment," applicable to terms or predicates in the degree to which they (or their extensional equivalents, that is, words picking out the same class of elements, like "triangle" and "trilateral") have actually been previously employed in projection: in formulating inductions on the basis of positive, though incomplete evidence. To illustrate with our previous example, the predicate "has been examined prior to t and conducts electricity or has not been examined prior to t and does not conduct electricity" is less well entrenched than the predicate "conducts electricity," *because the class it singles out has been less often mentioned in formuating inductions*. The factor of actual historical employment of constituent predicates or their equivalents can thus be used to distinguish between hypotheses such as S_1 and S_4, which are equal in point of available positive instances. Goodman appeals, then, to "recurrences in the explicit use of terms as well as to recurrent features of what is observed," suggesting that the features which we fasten on in induction are those "for which we have adopted predicates that we have habitually projected" (*1*, pp. 96, 97). With this idea as a guide, Goodman first defines presumptively projectible hypotheses. Next, he defines an initial projectibility index for these hypotheses. Finally, he defines degree of projectibility by means of the initial projectibility index as modified by indirect information embodied in what he calls "overhypotheses," *which must themselves qualify as presumptively projectible*. The latter use made of indirect evidence is worked out with great care and detail and is of independent theoretical interest.

Roughly, degree of projectibility is to represent what Goodman earlier called "lawlikeness," (that is, that property which, together with truth, defines scientific laws) and constitutes therefore not only an explanation but also a refinement of the latter. With the explanation of lawlikeness, Goodman suggests that the general problem of dispositions is solved. For this general problem is to define the *relationship* between "manifest" or observable predicates (for example, "dissolves") and their dispositional counterparts (for example, "is soluble") and manifest predicates may now be construed as related by true lawlike or projectible hypotheses to their dispositional mates. Other problems, such as the nature of "empirical possibility" are also illuminated by this approach, and some light is thrown on the difficult question of counterfactual judgments which, however, still resists full interpretation.

The most natural objection to Goodman's new approach is that it provides no explanation of entrenchment itself. In using this notion to explain induction, however, Goodman does not at all rule out a further explanation of why certain predicates as a matter of fact become entrenched while others do not. His purpose is to formulate clear criteria, in terms of available information, that will single out those generalizations in accordance with which we make predictions. The strong point of his treatment is that his criteria do indeed seem effec-

tive in dealing with the numerous cases he considers.

A possible misconception concerning the use of "entrenchment" as a basic idea is that it may lead to the ruling out of unfamiliar predicates, thus stultifying the growth of scientific language. Unfamiliar predicates may, however, be well entrenched if some of their extensionally equivalent mates have been often projected, and they may acquire entrenchment indirectly through "inheritance" from "parent predicates"—that is, other predicates related to them in a special way outlined in detail in Goodman's discussion (*1*, p. 105). Furthermore, Goodman's criteria provide methods for evaluating *hypotheses*, not predicates, so that wholesale elimination of new scientific terms is never sanctioned in his treatment.

As remarked previously, the critical discussion of Goodman's new approach is still in its early stages (*5*). His formulations will undoubtedly undergo further refinement and revision with continuing study, but even in their present form they will have contributed much toward putting important questions in the philosophy of science on a scientific basis.

References and Notes

1. N. Goodman, *Fact, Fiction and Forecast* (Harvard Univ. Press, Cambridge, Mass., 1955).
2. ———, "A Query on Confirmation," *J. Philosophy* **43**, 383 (1946); "The Problem of Counterfactual Conditionals, *J. Philosophy* **44**, 113 (1947).
3. See in particular R. Carnap, "On the Application of Inductive Logic," *Philosophy and Phenomenological Research* **8**, 133 (1947); N. Goodman, "On Infirmities of Confirmation Theory," *Philosophy and Phenomenological Research* **8**, 149 (1947); R. Carnap, "Reply to Nelson Goodman," *Philosophy and Phenomenological Research* **8**, 461 (1947).
4. J. S. Mill, *A System of Logic*. (Longmans, London, 1843; new impression, 1947), book III, chap. III, sect. 3, p. 205.
5. See, in this connection, the long study of *Fact, Fiction and Forecast* by J. C. Cooley [*J. Philosophy* **54**, 293 (1957)] and Goodman's reply [*J. Philosophy* **54**, 531 (1957)].

Robert H. Lowie, Anthropologist

With the death of Robert Lowie, on 21 September 1957, American anthropology lost an ethnographer and ethnologist who had earned international esteem during the richly productive course of his 74 years.

Ethnographically his interests were world-wide, and his command of the literature on primitive peoples was unmatched. He belonged to a generation whose research among tribal peoples was still opening up unsuspected ranges of social organization and human values. Testifying both to his self-discipline and to his enthusiasm, monograph after monograph on the Indians of the American plains appeared under his name. He wrote steadily, with a fine pen and in a flowing hand, manuscripts that required little revision. His name is permanently associated with scholarly records of the North American Indians: the Plains, the Shoshone, the Hopi. His early and enduring devotion was given primarily to the Crow Indians, whose language and people he prized. His early enthusiasm was given to his adventures among the Lake Athabasca Chippewa. Later, his interests turned to South America. Here he had no opportunity for field research, but his characteristic generosity and his interest in the work of others did much to advance knowledge of the primitive peoples of Brazil.

His concern with European ethnography was the last phase of his area interests and represented a return to his youth. Born in Vienna in 1883, he was brought to New York at about the age of ten, where he grew up as a bilingual in a middle-class Jewish intellectual milieu. Thus, his interest in languages and in bilinguality, as well as his bent for scholarship, were early established and deep. Similarly, he retained throughout his life a certain old-world courtliness.

During the second world war he began teaching European ethnography, and from this his interest in Germanic culture and society, with certain autobiographical implications, developed steadily. Yet he delighted, despite its anti-Germanic overtones, in Rebecca West's *Black Lamb and Grey Falcon*. On the other hand, his unremitting capacity for detail made him suspect facile theorizing in the "national character" school of anthropology that blossomed during and after the war. He set himself against such overgeneralizations in *The German People* (1945).

However, these successive periods of ethnographic interest—study of the North American Indians, the tribes of South America, and the German peoples—were evenly balanced by a desire to formulate broader ethnologic syntheses. *Primitive Society* appeared in 1920. Reissued and widely translated, it remains today a fundamental statement. *History and Theory of Anthropology* (1937), *Primitive Religion* (1924), and *Origin of the State* (1927) are only indices of his indefatigable, informed, and systematic intellect. He dealt with theories as if they were artifacts—dryly, carefully, factually. This approach was, and is, a salutary corrective to half-cocked enthusiasts.

Although Lowie was never personally as close to Boas as some of his contemporaries were, he was formed (in the French sense of the word) by that seminal genius. Although he had studied chemistry in his undergraduate days and had been an enthusiastic admirer of Karl Pearson, it was in anthropology that Columbia awarded him a Ph.D. degree in 1908. From that time his devotion to anthropology was unflagging. Nevertheless he found opportunities to express a youthful militancy in support of the feminist movement, encouraged by his lifelong friend Elsie Clews Parsons. And he participated in what must have seemed, to so nonpolitical a character, the innocuous ferment created by "the Masses" and John Reed. He always retained a warm memory of his liberalism of those early days.

From 1921 to 1950 he taught—and, more important, he trained, in critical objectivity—generation after generation of students at the University of California in Berkeley. He served for nine years (1924 to 1933) in the thankless post of editor of the *American Anthro*

pologist. In the last years of his life he confided to me that he felt some regret that he had created no positive school of followers. Yet his influence on all who studied with him was manifest in the standards of scholarship and objectivity that he set us.

He also set his students perhaps more important standards than these. He was a liberal gentleman in the deepest sense of both these words. In personal relations he never hesitated to give the best counseling of which he was capable, but he never resented disregard of that counsel. He was generous and utterly without malice, yet he enjoyed controversy where he suspected sham or pretension.

For those of us who held him in esteem and affection, it is consoling to know that he received honors commensurate with his qualities. He served as president of the American Anthropological Association in 1935; he was elected to the National Academy of Sciences; he was the Viking Medalist in 1947 and the next year gave the Huxley memorial lecture at the Royal Anthropological Institute.

After his retirement in 1950 he was much in demand as a visiting lecturer and professor, both in the United States and abroad. He died completed in honors, respect, and the affection of his wife and many friends. If, on occasion, he sensed that he had not permitted himself time for certain subtleties, this was only a measure of his stature. His life was fulfilled.

CORA DU BOIS
Harvard University

News of Science

The President on Science and Education

In his State of the Union Message to Congress on 9 January, President Eisenhower made the following statements about international scientific cooperation and about education and research:

"It is highly important that the Congress enact the necessary legislation to enable us to exchange appropriate scientific and technical information with friendly countries.

"It is wasteful in the extreme for friendly allies to consume talent and money in solving problems that their friends have already solved—all because of artificial barriers to sharing. And we cannot afford to cut ourselves off from the brilliant talents and minds of scientists in friendly countries. The task ahead will be hard enough without handcuffs of our own making.

"The groundwork for this kind of cooperation has already been laid in discussions among NATO countries. Promptness in following through with legislation will be the best possible evidence of American unity of purpose in cooperating with our friends.

"In the area of education and research, I recommend a balanced program to improve our resources, involving an investment of about a billion dollars over a four-year period. This involves new activities by the Department of Health, Education, and Welfare designed to encourage improved teaching quality and student opportunities in the interests of national security. It also provides a five-fold increase in the sums available to the National Science Foundation for its activities in stimulating and improving science education.

"Scrupulous attention has been paid to maintaining local control of educational policy, spurring the maximum amount of local efforts, and to avoiding undue stress on the physical sciences at the expense of other branches of learning.

"In the field of research, I am asking for substantial increases in basic research funds, including a doubling of the funds available to the National Science Foundation for this purpose.

"But federal action can do only a part of the job. In both education and research, redoubled exertions will be necessary on the part of all Americans if we are to rise to the demands of our times. This means hard work on the part of state and local governments, private industries, schools and colleges, private organizations and foundations, teachers, parents, and—perhaps most important of all—the student himself, with his bag of books and his homework.

"With this kind of all-inclusive campaign, we can create the intellectual capital we need for the years ahead—and do all this, not as regimented pawns, but as free men and women. . . ."

Toward the end of his address, the President discussed "works of peace," especially in connection with improving relations with the U.S.S.R.:

"Now, may I try to give you some concrete examples of the kind of works of peace that might make a beginning in the new direction. . . . We now have it within our power to eradicate from the face of the earth that age-old scourge of mankind: malaria. We are embarking with other nations in an all-out five-year campaign to blot out this curse forever. We invite the Soviets to join with us in this great work of humanity.

"Indeed, we would be willing to pool our efforts with the Soviets in other campaigns against the diseases that are the common enemy of all mortals—such as cancer and heart disease.

"If people can get together on such projects, is it not possible that we could then go on to a full-scale cooperative program of science for peace?

"A program of science for peace might provide a means of funneling into one place the results of research from scientists everywhere and from there making it available to all parts of the world.

"There is almost no limit to human betterment that could result from such cooperation. Hunger and disease could increasingly be driven from the earth. The age-old dream of a good life for all could, at long last, be translated into reality. . . ."

Brode Appointed Science Adviser to State Department

The Department of State has named Wallace R. Brode as science adviser and will soon appoint science attachés to the embassies in London, Bonn, Paris, Stockholm, New Delhi, and Tokyo. The department has $200,000 available to revive the scientific advisory program, which has been operating with a greatly reduced staff during the past 2 years (see p. 175). According to a State Department spokesman, the program will probably be expanded after it gets under way. Attachés will then be sent to European embassies other than those included in the initial list, as well as to embassies in Latin America and the Near East.

Although Brode will retain his presi-

dency of the AAAS, he will take a leave of absence from his post as associate director of the National Bureau of Standards and will resign from the Editorial Board of *Science*.

Polish Scientist Asks Political Asylum

Jerzy Leon Nowinski, Polish specialist in thermoelasticity, has been granted political asylum by the United States. Nowinski, who for the past 7 years has been a professor at the University of Warsaw, arrived here in November to serve as a guest professor at Johns Hopkins University. Shortly after learning that his wife and 7-year-old daughter had reached safety in England, he told the Federal Bureau of Investigation in Baltimore of his desire to stay in this country. His family will join him.

Nowinsky himself did not apply originally for his passport to come to the United States. After he had been invited by Johns Hopkins to be a guest lecturer in its graduate school, his colleagues in the Polish Academy of Science, where he was associate editor of the academy journal, urged the government to let him make the trip as a matter of scientific prestige.

In a press conference on 3 January, Nowinski, who is a Roman Catholic, explained his action by saying: "I was rather disappointed with the political, the moral, and the religious conditions in Poland after the war. Also, our child had to attend school and my wife and I decided she must attend a school with better religious and moral conditions."

Although he was treated well because of his scientific position in Poland, Nowinski commented: "It is this feeling of freedom we feel so strongly in the United States that compares with the difficult problem of living in Poland."

When asked for his opinion about whether or not Russia was ahead of the United States in developing an intercontinental ballistic missile, Nowinski said that he was not in a position to know. He would not say what contribution he might make to this country's missile program, but said that he would be willing to work in that area if asked.

Meteorologists Object to New Civil Service Pay Rates

In accord with a department circular from the U.S. Civil Service Commission on 9 December 1957, increased minimum pay rates have been established for professional engineers and certain scientists through grade GS-17 [*Science* **127**, 21 (3 Jan. 1958)]. Noting the omission of meteorologists from the positions listed under the amended salary scale, the American Meteorological Society has sent the following telegram to Harris Ellsworth, chairman of the U.S. Civil Service Commission; John W. Macy, Jr., executive director of the Civil Service Commission; Sinclair Weeks, Secretary of Commerce; Neil H. McElroy, Secretary of Defense; James R. Killian, Jr., Special Assistant to the President for Science and Technology; and to the chairmen of the Senate and House Post Office and Civil Service Committees:

"The American Meteorological Society deplores the exclusion of meteorologists from the salary adjustment recently announced for scientists and engineering personnel in Civil Service. The society feels that the long-term objectives of attracting promising young men and women to the meteorological profession and to important work in civil and defense science will best be achieved by eliminating salary differentials that impose a financial penalty on scientists and professional people selecting meteorology as their primary field of interest. The nature and the importance of the scientific problems in meteorology and the urgency of further advances in this field are sufficiently well known in our opinion to merit a reexamination of this salary policy. We respectfully request reconsideration of the directive excluding meteorologists from this salary adjustment program."

AAAS Cardiovascular and Socio-Psychological Awards

Irvine H. Page, head of the Research Division, Cleveland Clinic, Cleveland, Ohio, has received the AAAS Ida B. Gould Memorial Award for Research on Cardiovascular Problems. The $1000 prize, which is sponsored by the Richard and Hinda Rosenthal Foundation, was given for the second time at the Association's recent meeting in Indianapolis.

Early in his career, Page spent 3½ years working on the chemistry of the brain; the results, along with a survey of the literature, appeared in *Chemistry of the Brain*, the second book on the subject ever published. Page completed a number of papers on the chemistry of phosphatides and on the synthesis of a large series of cholesterol esters; then, in 1929, he began to study the chemistry of arteriosclerosis.

At the Rockefeller Institute (1931–37) Page worked with D. D. Van Slyke on the chemical substances in blood and tissue that control the caliber of blood vessels, hence blood pressure. This investigation culminated in the discovery, with Helmer, of the peptide angiotonin (now called angiotensin). This occurred after Page had gone to Indianapolis, where he directed the Lilly clinic and laboratory for clinical research at Indianapolis City Hospital from 1937 to 1945.

Thirteen years ago Page and his group moved to the Cleveland Clinic Foundation, where they have been since. During the war Page, with his close associate Corcoran, was occupied with the problem of shock. Transfusion of blood into arteries instead of veins was studied with Kohlstaedt, and this method of transfusion still has important applications. Page's group also found that one of the important components of the shock mechanism is failure of the blood vessels to respond to stimulation or loss of cardiovascular reactivity. In addition, a substance was isolated in the blood of shocked dogs that caused severe contraction of blood vessels.

After the war, work on the constrictor substances in the blood was continued, and a method for testing the constrictor substance that forms when blood coagulates was elaborated and some of the properties of this substance determined. This resulted in a study with Rapport and Green that led to the isolation and crystallization of serotonin. Rapport determined the final structure as 5-hydroxytryptamine; this was synthesized by Hamlin. Page's subsequent studies on serotonin, like those of many others, have shown it to be concerned with a variety of functions of the body, including intestinal motility, stopping of bleeding, transmission of nerve impulses, pain, and functioning of the brain.

Page, with Corcoran and Dustan, contributed a long series of studies on hypertensive patients having diseases produced by such drugs as hydralazine, hexamethonium, and mecamylamine. Angiotensin was synthesized, with Schwarz and Bumpus, last year and its properties studied in animals [*Science* **125**, 886 (3 May 1957)]. There is little doubt that in some types of hypertension angiotensin is the substance that raises the blood pressure.

The Cleveland group has carried out extensive studies on the chemical changes associated with arteriosclerosis. For example, the importance of the β-lipoproteins in the blood for the production of arteriosclerosis has been demonstrated.

* * *

Irving A. Taylor, assistant professor of psychology at Pratt Institute, was awarded the 1957 AAAS Socio-Psychological Prize of $1000 at Indianapolis for his essay on "Similarities in the Structure of Extreme Social Attitudes." Taylor has been engaged in study of this subject for the past 5 years. There is an apparent trend in certain areas of psychological literature to regard what has been frequently called the "authoritarian" and "equalitarian" attitudes as polar opposites with contrasting clusters of social characteristics. The purpose of Taylor's

prize-winning study was to test the hypothesis that extreme authoritarians and equalitarians would exhibit an important perceptual similarity.

The California 30-item F scale and a social distance scale containing ethnic minority groups was administered to 253 summer students at the University of Houston along with perceptual closure scales designed specifically for the study. Subjects scoring in the same relative category—that is, in the low, middle, and high quarters on both the F scale and the social distance scale—were selected and compared with regard to percentage of perceptual closure. The results indicated a statistically significant curvilinear relationship; high and lows reduced the size of the openings by only 12 percent. Whereas the difference between the extremes was not significant, the difference between either extreme and the middle was significant at the 5-percent level of confidence.

Examination of population characteristics also revealed a surprising number of similarities between extremes. Although not all of these findings were statistically significant, the practically exceptionless tendency for extremes to vary together was highly indicative. The extreme groups, for example, were found to be somewhat older and of a higher level of education than the middle group. There were substantially more women in the extreme categories than men. Both extremes reported significantly lower parental incomes than did the middle. These findings, if confirmed in subsequent research, would tend to modify the prevalent beliefs that authoritarianism increases with age, that women tend to be more equalitarian in attitude than men, and that extreme equalitarians come from lower economic levels. In summary, then, it was found that the extremes were more similar to each other than either one was to the middle group with regard to perceptual and demographic characteristics.

The results of this study indicate the need for re-examining the generally held view that the attitude structures of the extreme authoritarian and equalitarian are essentially different. They also indicate that the results of studies reporting differences where only the California scales and the two extremes are considered are in need of re-examination.

Nuclear Progress Summarized

The Atomic Industrial Forum recently published "The Atomic Industry—1957," its annual progress report on developments in commercial applications of atomic energy during the year. The U.S. atomic industry completed the construction of 16 nuclear reactors, including seven power-type reactors and nine research and test reactors, for both private and governmental purchasers in the United States and abroad, according to the report. The power-type reactors include those designed for naval ship propulsion and for prototype and demonstration power plants. In addition to these projects, industry continued or began the construction of 59 reactors, 35 of which are power-type reactors and 24 of which are research and test reactors, and received orders for the manufacture of ten new reactors—five for the production of electric power and five for research and test purposes.

Also, according to the report, American industry signed contracts for the construction of seven uranium ore processing mills, 13 companies announced plans to enter the field of nuclear fuel element production and research, one company began construction of a privately owned feed materials plant, three companies completed construction of plants for the production of zirconium, and two companies completed construction of plants for the production of beryllium.

Grants, Fellowships, and Awards

Arctic Research. The Carnegie Corporation of New York supports scholarships which are tenable at McGill University, Montreal, and which are normally offered to students proceeding to a doctoral degree in a subject calling for active field research in arctic or subarctic North America. Candidates who do not intend to proceed to a degree are not necessarily disqualified. The awards have an average value of $1500 for the academic session and $1250 for the expenses of a summer's field expedition. If renewed for a second session, grants average $1750.

Applications should be submitted before 1 March to the Secretary of the Carnegie Arctic Program, McGill University, 539 Pine Ave. W., Montreal, Canada, and should include a confidential recommendation of the candidate's qualifications in his or her selected field and a clear statement of the intended arctic or subarctic research project. No particular form of application is required.

Science Teaching. Nominations for Science Teacher Achievement Recognition awards designed to stimulate and recognize superior laboratory instruction in science in grades 7 through 12 in public, private, and parochial schools in the United States must be submitted before 15 February to the National Science Teachers Association, 1201 16th St., NW, Washington 6, D.C. Entries must be based on creative, laboratory-type procedures that may be utilized in the effective teaching of science. Recognition will be in the form of cash awards, medallions, plaques, and certificates of merit. The program is conducted by NSTA under a grant from the National Cancer Institute, U.S. Public Health Service.

Zoological Nomenclature

The International Commission on Zoological Nomenclature has announced that, beginning 30 June 1958, it will start voting on the following cases involving the possible use of its plenary powers for the purpose specified against each entry. Full details of these cases were published on 30 December 1957 in the *Bulletin of Zoological Nomenclature* (vol. 13, parts 10/11, and vol. 16, part 1): (i) *Selene* Lacépède, 1803; *rostrata* Lesueur, 1817 (*Muraena*); *latipinna* Lesueur, 1821 (*Mollienesia*); *fuscus* Storer, 1839 (*Syngnathus*); establishment of precedence of, over other names published in the same work and on the same date (Cl. Pisces). (ii) *Monograptus fimbriatus* var. *similis* Elles (G.L.) & Wood (E.M.R.), 1913; *Monograptus triangulatus* var. *major* Elles & Wood, 1913; *Monograptus communis* var. *rostratus* Elles & Wood, 1913; designation of lectotypes for (Cl. Graptolithina). (iii) *Calandra* (*Calendra*) Clairville & Schellenberg, 1798, suppression of, in favor of *Sphenophorus* and *Sitophilus*, both of Schoenherr, 1838, respectively, in interests of universality of nomenclature; *abbreviatus* Fabricius, 1787 (*Curculio*) and *oryzae*, emendation to of *oryza* Linnaeus, 1763 (*Curculio*), validation of (Cl. Insecta, Order Coleoptera).

Comments should be sent as soon as possible in duplicate to the secretary of the commission, Francis Hemming, 28 Park Village East, Regent's Park, London, N.W.1, England.

Scientists in the News

ERNEST H. VOLWILER, president and general manager of Abbott Laboratories, North Chicago, Ill., and a leader in medicinal chemistry, has won the 1958 Priestley Medal of the American Chemical Society. The gold medal, highest honor in American chemistry, will be presented to Volwiler for "distinguished services to chemistry" at the society's 133rd national meeting in San Francisco in April.

Volwiler's first contribution to medicinal chemistry was the commercial production of anesthetics during World War I, after German sources for these supplies were cut off. He developed manufacturing techniques for such anesthetics as Anesthesin, Benzocaine, and Novocaine. Later Volwiler's efforts to improve synthetic drugs resulted in the

development of Butyn, a non-narcotic pain killer. He also was responsible for the production of Nembutal, a hypnotic-sedative, and Pentothal, a general anesthetic.

During World War II, the sulfa drugs sulfanilamide, sulfathiazole, and sulfadiazine, and the antibiotic penicillin were produced under his direction. At the close of the war he was selected by the United States Surgeon General and the Chemical Warfare Service to head a team of scientific investigators that was sent to Germany to survey the chemical industry there. The investigators brought back valuable information on Methadone, a powerful analgesic, and polyvinylpyrrolidone (PVP), a compound that was later adapted as a blood plasma extender.

Volwiler's work in evaluating new chemical compounds and in evolving large-scale manufacturing processes was a major factor in bringing about the commercial production of Sucaryl, a synthetic sweetening agent for diabetics and persons on low-calorie diets.

I. D. WILSON, head of the biology department of Virginia Polytechnic Institute since 1923, retired from that post on 31 January. He has accepted a 2-year contract from the International Cooperation Administration to be a consultant to universities in India.

WALTER R. MILES has arrived at the U.S. Naval Medical Research Laboratory in New London, Conn., to assume the recently established post of chief scientist. As scientific director, he will be the top-ranking civilian in the laboratory, and will be assistant to the officer-in-charge in all research matters. He will coordinate and direct the scientific program within the six branches of the laboratory, represent the laboratory at scientific meetings, and act as liaison officer and consultant to the Navy's Bureau of Medicine and Surgery for the laboratory.

Miles has recently returned from Istanbul, Turkey, where he served for 3 years as Ordinarious professor of psychology at the University of Istanbul. Prior to that, he was for many years professor of psychology at the Yale University School of Medicine.

The following scientists from Australia are visiting North America.

G. M. WATSON, medical research officer for the Australian Atomic Energy Commission, arrived on 19 January. His itinerary includes Washington (19–26 January), Oak Ridge, Santa Fe, San Francisco, Chicago, New York, Boston, Ottawa, and Montreal, and he will depart for London about 6 March.

L. A. T. BALLARD of the Division of Plant Industry, Commonwealth Scientific and Industrial Research Organization, is at the University of California, where he will spend approximately 10 months with K. C. Hamner in the department of botany. Ballard is interested in developmental physiology.

J. R. PHILIP, also of the C.S.I.R.O. Division of Plant Industry, is spending some 14 months in the Division of Biology of California Institute of Technology working on mathematical and physical aspects of plant environments and processes.

Scientific visitors to North America from England are:

BRIGIT A. ASKONAS, member of the National Institute for Medical Research (Medical Research Council), Mill Hill, London, is visiting a number of centers between 15 January and 25 February to study tissue-culture technique in relation to immunology. Her itinerary includes Boston, New York, Baltimore, Bethesda (Md.), West Point (Pa.), Cleveland, and Toronto.

J. C. KENDREW, member of the unit for research on the molecular structure of biological systems (Medical Research Council), Cavendish Laboratory, Cambridge, recently attended a meeting at the National Institutes of Health, and will also attend the conference of the Biophysical Society of America in Boston, 5–7 February.

E. E. POCHIN, director of the Medical Research Council's department of clinical research, University College Hospital Medical School, London, will attend the meeting in New York of the United Nations Scientific Committee on the Effects of Atomic Radiation. He will depart sometime in February.

FREDDY HOMBURGER and PETER BERNFELD have founded Bio-Research Institute, Inc., and Bio-Research Laboratories, Inc., independent, nonprofit organizations for fundamental biological and biochemical research and for the study of man-made environmental causes of disease. New laboratories were opened on 1 January at 9 Commercial Ave., Cambridge, Mass. The two men, who were previously research professor of medicine and associate professor of biochemistry, respectively, at Tufts University School of Medicine, will also offer services in consultation, research, and development to the drug, cosmetic, and food industries through the firm of Bio-Research Consultants, located at the same address.

HARRY H. SOBOTKA of Mount Sinai Hospital, New York, will receive the second annual Van Slyke Award of the New York Section of the American Association of Clinical Chemists on 11 February at the New York Academy of Sciences. His award lecture will be devoted to "The Microbiological Assay of Vitamins in Clinical Chemistry." Donald D. Van Slyke of Brookhaven National Laboratory, for whom the award is named, will be chairman of the presentation meeting.

Recent Deaths

FRANZ F. BERG, Middlesex, N.J.; 64; associate director of pharmaceutical development in the Fine Chemical Division of the American Cyanamid Company; previously with E. R. Squibb & Son, Brooklyn, for 32 years; 30 Dec.

WILLIAM T. BOVIE, Fairfield, Me.; 75; retired biophysicist who perfected the electric scalpel and collaborated with Harvey Cushing in developing operating procedures for brain surgery; taught at Antioch College, Harvard University, Northwestern University, and Colby College; 1 Jan.

ARTHUR J. EWINS, London, England; 75; research chemist who was one of the discoverers of M and B 693, the antipneumonia drug; director of research at the laboratories of May and Baker, Ltd., manufacturing chemists of Dagenham, Essex, where he had worked since 1917; 30 Dec.

DWIGHT D. GARDNER, Columbus, Ohio; vice president of Commercial Motor Freight, Inc.; first elected president of the American Institute of Industrial Engineers; 17 Dec.

DOUGLAS M. KELLEY, Berkeley, Calif.; 45; professor of criminology at the University of California; chief psychiatrist during the Nuremberg Nazi war crimes trials and specialist in lie detection; 1 Jan.

ERNEST KENNAWAY, London, England; 76; professor emeritus of experimental pathology at the University of London and former director of the Chester Beatty Research Institute of the Royal Cancer Hospital; 1 Jan.

FREDERICK W. MERRIFIELD, Wilmette, Ill.; 70; professor emeritus of surgery at Northwestern University Dental School; 6 Jan.

LINDSAY R. PARKER, New York, N.Y.; 79; retired public utilities executive and electrical engineer; 1 Jan.

SIMON L. RUSKIN, New York, N.Y.; 60; chemist, inventor, and physician who specialized in the nose and throat; patented a procaine penicillin preparation; 1 Jan.

MARGARET H. SMYTH, Palo Alto, Calif.; 84; psychiatrist who had served as director of the Stockton State Mental Hospital and who was voted California's most distinguished woman in medicine at the Golden Gate International Exposition in 1940; 30 Dec.

KOICHI UDA, Tokyo, Japan; 53; lawyer, former minister in charge of atomic affairs in the Japanese Cabinet; 30 Dec.

Reports

Extra-Atmospheric Cosmic Ray Dosage during the Large Solar Flare of 23 February 1956

During the large solar flare of 23 February 1956 cosmic ray measurements were carried out on a world-wide scale. Though most of the measurements were collected at sea level and mountain altitudes, synoptic evaluation of observations made at different geomagnetic latitudes permits inferences to be drawn about the extra-atmospheric intensity of the primary cosmic ray beam during the flare. This intensity was so large that it seems of interest to investigate what ionization dosage might be encountered if human beings should be caught by a large solar flare in extra-atmospheric flight.

The neutron intensity of cosmic radiation, at sea level, as recorded among others by Lockwood and collaborators (*1*) in Durham, N.H., showed on the critical day a steep increase to 36 times its normal value 21 minutes after the onset of visible activity on the sun and then slowly returned to normal in about 20 hours. Specifically, at 20:30 Universal Time, somewhat over 16 hours after the maximum, the intensity had declined to a value about 15 percent above normal. At exactly the same time, the balloon flight of the University of Minnesota, as reported by Winckler (*2*), had reached an altitude of 80,000 feet and recorded a 4-times normal total ionization. From these data two conclusions can be drawn: (i) If a 300-percent increase of the total ionization at 80,000 feet is reflected in a 15 percent increase of neutron intensity at sea level, the increase of the total ionization at extreme altitude at the time of the cosmic ray maximum when the sea-level neutron intensity was 36 times normal must have been tremendous. (ii) The disproportionately smaller intensity change at sea level indicates that the flare-produced radiation is of low penetrating power—that is, it mainly affects low-energy particles.

More detailed estimates of the additional intensity produced by the flare at the top of the atmosphere have been made by Meyer, Parker, and Simpson (*3*). From their data on the neutron intensity at sea level and at mountain altitudes in different geomagnetic locations, they were able to analyze the energy spectrum of the flare-produced primary protons in the cosmic ray beam. This spectrum exhibits a very strong dependence of particle intensity on geomagnetic latitude, approximately following a N^{-7} power law (N = magnetic rigidity). It is interesting to align the flare-produced and the normal spectrum in one graph (Fig. 1). The normal spectrum shown is that suggested by Winckler (*4*). The flare spectrum is derived by Meyer, Parker, and Simpson from observations at the geomagnetic latitudes 29°, 42°, 48°, and 52° marked by arrows on the abscissa. In trying to extrapolate these data to lower magnetic rigidities—that is, to higher latitudes—the phenomenon of the so-called geomagnetic cutoff has to be considered. The low-rigidity end of the normal integral spectrum is known to shift to smaller rigidities with increasing latitude, in full agreement with geomagnetic theory, up to a certain medium-high latitude. From there on it remains constant, contrary to expectation. In the normal spectrum in Fig. 1, this minimum cutoff latitude is assumed to be 54°, corresponding to a minimum rigidity of 1.78 Bv. However, the observations of different experimenters do not agree well with regard to the exact value of this critical latitude. Meyer, Parker, and Simpson draw the conclusion from their flare observations that "the primary spectrum was strongly peaked at particle rigidities just above the geomagnetic cutoff rigidity at Chicago" (*3*, p. 773). This would set the latitude knee of the flare spectrum just below 52°, as indicated in Fig. 1. Of particular interest in this connection is the fact that the cutoff phenomenon shows a strong correlation to solar activity in the sense that it is very weak or nonexistent at solar minimum during the 11-year cycle. This relationship would represent a protective mechanism inasmuch as large solar flares occur only at times of high solar activity—that is, at times when the cutoff is fully developed, preventing low-energy particles from reaching the earth.

For a radiobiological assessment of the flare-produced radiation, the total extra-atmospheric dosage should be expressed in rad units. This conversion can easily be carried out and is indicated in the right-hand ordinate index of Fig. 1. The extremely strong dependence of this dosage on geomagnetic latitude suggests that any indirect way of assessing the exposure hazard in quantitative terms must be subjected to a large margin of error. Direct measurements seem indispensable for the flare-produced radiation more than for any parameter of the ordinary cosmic ray beam. If the flare-produced intensity levels off at 52°, the dosage for the polar region would stop growing just below the danger mark. However, it remains an open question whether or not, outside magnetically shielded regions, the flare spectrum follows the straight-line extrapolation shown in Fig. 1. The implications of this possibility for the exposure hazard in interplanetary space during a large flare are ominous.

Entirely missing are clues on the participation of heavy nuclei in the flare-produced transitory radiation. This is particularly unfortunate since the biological effects of the ordinary cosmic ray beam center strongly on low-energy heavy nuclei of maximum ionization (so-called "thindown hits"). The complementary probabilities of a thindown hit or a nuclear collision for heavy nuclei are plotted in the upper graph of Fig. 1. When the speculative assumption is introduced that heavy nuclei in a solar flare are accelerated in a manner similar

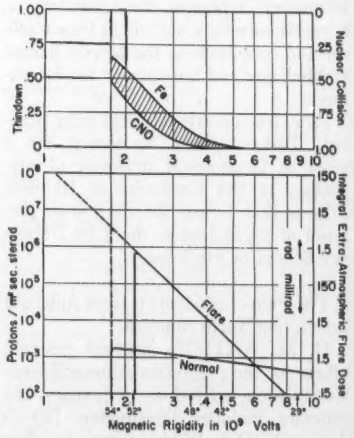

Fig. 1. (Bottom) Normal and flare-produced integral rigidity spectrum of primary protons. (Top) Probability of termination in thindown or nuclear collision for heavy nuclei as a function of magnetic rigidity.

All technical papers and comments on them are published in this section. Manuscripts should be typed double-spaced and be submitted in duplicate. In length, they should be limited to the equivalent of 1200 words; this includes the space occupied by illustrative or tabular material, references and notes, and the author(s)' name(s) and affiliation(s). Illustrative material should be limited to one table *or* one figure. All explanatory notes, including acknowledgments and authorization for publication, and literature references are to be numbered consecutively, keyed into the text proper, and placed at the end of the article under the heading "References and Notes." For fuller details see "Suggestions to Contributors" in *Science* 125, 16 (4 Jan. 1957).

to that in which protons are accelerated, it is seen that, in this case, the biologically most important fraction of the heavy beam which produces the thin-down hits would be subjected to an especially large increase of intensity. It might be mentioned in this connection that the optical emission lines of all heavy components, particularly those of calcium and iron, have been identified in the flare spectrum.

Other parameters on which no data are available are the intensities of flare-produced beta, x-, and gamma rays. It thus appears that, while the extra-atmospheric ionization dosages plotted in Fig. 1 certainly represent the lower limit, they might possibly do so by a large margin. On the other hand, flares of the size under discussion are rare events that occur only a few times during the entire peak of one 11-year cycle.

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26 August 1957

Concentration of Cesium-137 by Algae

The ab-adsorption of Cs^{137} by algae is of interest because Cs^{137} is one of the critical fission products in power reactor wastes and atomic weapon fallout, because it has an estimated half-life of 26.6 years, because it is water-soluble, and because it may be expected to increase in the environment as more and more use is made of nuclear energy as a source of electric power. It is well known that plankton take up radioactivity in fairly high concentrations (*1*). The purpose of this investigation was to study the accumulation of Cs^{137} by fresh-water algae (*2*).

Unialgal cultures of nine species, collected from the vicinity of Oak Ridge National Laboratory, were dosed with Cs^{137} chloride so that the initial activity of the nutrient media ranged from 1556 to 4056 disintegrations per minute, per milliliter. Results from these nine species are given in Table 1. The concentrations (in parts per million) of potassium present in the nutrient media were determined by flame photometry. The concentration factor given in Table 1 is the ratio of the activity of wet weight of washed algal cells (disintegrations per minute, per gram) to the activity of the nutrient medium (disintegrations per minute, per milliliter). The remainder of this report deals with the accumulation of cesium by *Euglena intermedia* and *Chlorella pyrenoidosa*. Dense populations were produced repeatedly in a putrified "Euglena" nutrient medium (*3*); white fluorescent lamps, delivering about 380 ft-ca, controlled by automatic clocks to give a 20-hour day and a 4-hour night, were used. The extent to which these algae decontaminate their nutrient medium was investigated. In 6 days *Euglena* decontaminated the medium by 69 percent; in 11 days, by 82 percent; in 18 days, by 86 percent; and in 34 days, by 96 percent. *Chlorella* decontaminated the medium by about 47 percent at each concentration of Cs^{137} in 13 days. The rate of decontamination appeared to vary directly with the number of cells in *Chlorella*, but in *Euglena* there was an increase in uptake per cell from the time cells ceased dividing, as the population became maximum.

Unialgal cultures of *Euglena* and *Chlorella*, grown in three concentrations of Cs^{137}, demonstrated repeatedly that the uptake of this radionuclide is linear with concentrations of 1, 5, and 10 μc/lit or of 1, 5, and 10×10^{-7} mmole/lit. Knauss and Porter (*4*) have found this to be true for calcium, iron, manganese, zinc, copper, and strontium at varied concentrations in *Chlorella*. However, at concentrations of 0.04 mmole/lit or more, in the present study, the concentration factor for cesium was reduced.

Morgan and Myers (*5*) have reported that uptake of cesium by *Chlorella* is significantly depressed by traces of potassium ion in the exchange solution. Also, cesium has been reported by MacLeod and Snell (*6*) to behave similarly to sodium, potassium, and rubidium in the nutrition of lactic acid bacteria. The relative influence of potassium and stable cesium on the uptake of tracer cesium by *Euglena* was determined experimentally, and the results are shown in Fig. 1. In the range of concentrations from 0.5 to 4.0 mmole/lit, potassium had a slight depressant effect on the uptake of Cs^{137} at tracer levels. When 1 mmole of potassium was present, increasing the concentrations of cesium carrier from 0 to 0.15 mmole/lit depressed the uptake of tracer cesium. The further increase of cesium content to 0.75 mmole/lit caused little additional depression of Cs^{137} uptake. It should be noted that an addition of an equimolar amount of potassium resulted in very little additional depression of the cesium uptake. The effect of potassium in the medium is presumed to be slight, because a concentration factor of 100 was found for *Chlorella* grown in media with potassium concentration as high as 1 mmole/lit.

In experiments during which *Chlorella* was grown in varying concentrations of stable cesium, the concentration factor for Cs^{137} was 46 to 59 in the absence of cesium, 15 to 18 with 0.5 mmole of stable cesium per liter, and 8 to 12 for other concentrations of stable cesium up to 2.5 mmoles per liter.

Table 1. Uptake of cesium-137 by species of algae.

Species	Potassium (parts per million)	Days after dosing	Concentration factor
Rhizoclonium hieroglyphicum	1	5	1530
Oedogonium vulgare	1	3	790
Spirogyra ellipsospora	1	2	341
Spirogyra communis	13	5	220
Gonium pectorale	10	2	138
Oocystis elliptica	10	10	670
Chlamydomonas sp.	8	5	52
Euglena intermedia	8	14	706
Chlorella pyrenoidosa	8	11	154

Since it has been shown that dead organic material can have a high affinity for cesium (*7*), *Chlorella* and *Euglena* cells which had been grown in a medium containing 15 parts of potassium per million were washed, killed in formalin, and dosed with Cs^{137}. After 8 days the concentration factor was 16 for *Euglena* and 418 for *Chlorella*. Because dead *Chlorella* showed a high concentration factor for cesium, an experiment was performed to test the uptake of cesium from varying levels of cesium, potassium, and cesium-potassium by these dead cells. Healthy living cells of *Chlorella* were washed and centrifuged in demineralized water. These cells were then killed with formalin, equally distributed into 33 large centrifuge tubes, and resuspended in demineralized water. To the 33 tubes were added stable salts to make three classes of 11 each, as follows: (i) 0 to 5 mmoles of potassium per liter; (ii) 0 to 5 mmoles of cesium per liter; and (iii) potassium and cesium in varying ratios but always totaling 2.5 mmole/lit. Each tube was dosed to give an activity of about 1 μc of Cs^{137} per liter and left standing for 72 hours. Thereupon, the cells in each tube were

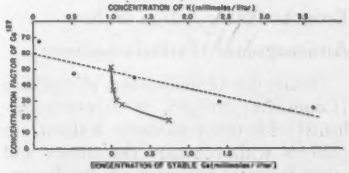

Fig. 1. Influence of potassium and cesium ions in the nutrient medium on the concentration of Cs^{137} by *Euglena intermedia*. The broken line represents the influence of potassium (regression line determined by least squares). The solid line represents the influence of cesium (curve fitted by eye), with potassium constant at 1.0 mmole/lit.

centrifuged, washed, dried, weighed, and analyzed for radioacitvity. The dead cells of *Chlorella,* with no added salts and at all levels of potassium (i), were found to concentrate Cs^{137} by a factor varying from 35 to 68; the variations appeared to be random. In mixtures containing stable cesium, (ii) and (iii), the concentration factor varied around 1—that is, the algal cell bodies contained the same amount of Cs^{137} as did an equal amount of the medium.

These results suggest that structural components persist in dead *Chlorella* which adsorb cesium from very dilute solutions and that this adsorption is not affected by the concentration of potassium in the medium. From these data it may be inferred that, in killed cells of *Chlorella,* potassium and cesium behave independently, but that in live cells of *Chlorella* and *Euglena,* particularly at tracer levels of potassium, ions of potassium and cesium form a metabolic pool.

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* Research participant of the Oak Ridge Institute of Nuclear Studies, summers, 1955 and 1957.

23 September 1957

Bronchodilator Action of the Anticoagulant Warfarin Sodium

During the administration of warfarin (Coumadin) sodium, or 3-(α-acetonylbenzyl)-4-hydroxycoumarin sodium, to patients with coronary thrombosis and other forms of thromboembolic disease, Livesay (*1*) noticed improvement in the asthmatic condition of several patients who had bronchial asthma in addition to the thromboembolic involvement. This observation prompted us to look for a direct bronchodilator action of warfarin sodium. For this purpose we employed the isolated guinea pig tracheal chain (*2*), a method which has been used extensively in pharmacological studies and which has been found to correlate well with clinical bronchodilator activity.

The results are shown in Table 1. Aminophylline, a well-established clinical bronchodilator, was used as a reference standard for the kymographic recording of relaxation or dilatation of the uncontracted tracheal chain. The lever system was adjusted so that 5 mg of aminophylline in the 100-ml bath, or a bath concentration of 0.05 mg per milliliter, produced a fall of about 2 cm on the kymograph tracing. Warfarin sodium was indeed found to possess some tracheodilator activity, being about 50 percent as active as aminophylline. By comparison, two different commercial samples of heparin sodium were found to be very weak, only about 5 percent as active as aminophylline. Since warfarin sodium and heparin sodium are sometimes injected simultaneously to secure the immediate anticoagulant effect of heparin and to initiate the slower but more prolonged effect of warfarin, a combination of equal amounts of the two drugs was tested on the tracheal chain. The tracheal dilatation was again approximately 50 percent that of aminophylline, demonstrating that the dilator effect of warfarin sodium was not influenced by the simultaneous presence of the heparin.

It has been pointed out (*3*) that some of the beneficial action of anticoagulants in myocardial infarction may be due to properties other than that affecting coagulation. The initial dose of warfarin sodium is usually 75 mg by intravenous, intramuscular, or oral administration, as compared with an aminophylline dosage of 250 to 500 mg intravenously or intramuscularly for emergency bronchodilatation and 100 to 250 mg orally for nonemergency use. If the dilator effect demonstrated on the tracheal chain is reflected in a corresponding bronchodilatation in man, it seems possible that the initial injection of warfarin sodium may produce some immediate bronchodilatation as well as initiating the slower anticoagulant action. Perhaps prolonged administration of small oral maintenance doses might also account for a bronchodilator effect by the same mechanism.

It is interesting to note, in this connection, that a coronary dilator action has been reported in dogs following intravenous injection of the disodium salt of bishydroxycoumarin (Dicumarol) (*4*) and of solubilized ethyl biscoumacetate (Tromexan) (*5*). Owren (*6*) has noted an improved effort tolerance from long-term anticoagulant therapy in patients with angina pectoris. The coronary dilator activity of warfarin sodium is yet to be investigated.

Studies will be extended to other anticoagulants to determine whether the observed warfarin sodium tracheodilatation or bronchodilatation is a general property of 4-hydroxycoumarin anticoagulants.

Table 1. Tracheodilator potency of anticoagulants, as compared with that of aminophylline.

Source	Bath concn. for 2-cm lever fall (mg/ml)	Approximate tracheodilator potency
Aminophylline		
10-ml ampule	0.05	100
Warfarin sodium		
Powder	0.1	50
Warfarin sodium		
3-ml vial	0.1	50
Heparin sodium–A		
10-ml vial	1.0	5
Heparin sodium–U		
10-ml vial	1.0	5
Warfarin sodium plus heparin sodium–U		
Vials—equal wts.	0.1 plus 0.1	50

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5 September 1957

Action of Selected Redox Substances on Bacterial Bioluminescence

Bioluminescence in *Achromobacter fischeri* and other luminous bacteria (*1*) depends upon a series of electron transfer reactions. The demonstration by Strehler (*2*) of bioluminescence in cell-free extracts of *A. fischeri,* and subsequent studies on the properties of this system, reviewed by McElroy and Strehler (*3*), showed that its essential components are reduced flavin mononucleotide ($FMNH_2$), a higher fatty aldehyde, from C_6 to C_{16}, atmospheric oxygen, and an extract of bacterial enzymes. Substrate and phosphopyridine nucleotide-specific dehydrogenase, reduced di- or triphosphopyridine nucleo-

tides (DPNH, TPNH), or reduced redox substances with potentials more negative than −81 mv can serve as electron sources for the reduction of flavin mononucleotide (FMN) (*4*).

Redox substances that react with the electron carriers involved in bioluminescence divert the electron flow from the light reaction and thus inhibit bioluminescence. For example, Spruit and Schuiling (*5*) found marked inhibition of bacterial luminescence by certain naphthoquinones and dyes whose redox potentials range from −33 to +65 mv, and McElroy and Strehler (*3*) point to the inhibition of luminescence by riboflavin or flavin adenine dinucleotide, substances which are thought to compete with FMN for the electrons from DPNH; a similar action is exerted by ferricyanide (*4*).

The study described in this report deals with the action on bacterial luminescence of redox indicators whose potentials range from −125 to +217 mv and shows that luminescence was inhibited by substances with redox potentials between +11 and +217 mv.

Suspensions of *A. fischeri*, prepared as described previously (*6*), were diluted 1:1.5 with 2.5 percent NaCl, buffered with 0.06*M* phosphate at *p*H 7.4. To these suspensions in test tubes were added the redox substances listed in Table 1. Bacterial luminescence and reduction of the indicator dyes were observed visually.

Reversible redox substances with potentials lower than +11 mv did not influence the luminescence of the bacteria, while above this potential level, luminescence was abolished and the dyes were reduced to their colorless leuco forms except for a narrow zone at the surface of the experimental mixtures.

The irreversible reduction indicator, triphenyltetrazolium chloride (TTC), was reduced to the red, water-insoluble triphenyl formazan by suspensions of *A. fischeri*, while bioluminescence was strongly but not completely inhibited. Triphenyltetrazolium chloride is known to be reduced by bacterial flavoproteins (*7*).

Table 1. Interaction of reversible redox substances with the bioluminescence of *Achromobacter fischeri*.

Dye and molar concn.	Redox potential (mv)	Inhibition of luminescence	Reduction of dye
Nile blue (9.1×10^{-5})	−125	−	−
Indigo tetrasulfonate (4.5×10^{-5})	−46	−	slight
Methylene blue (1.0×10^{-4})	+11	+	+
Thionine (1.5×10^{-4})	+62	+	+
Toluylene blue (1.1×10^{-4})	+115	+	+
2,6-Dichlorphenol indophenol (1.2×10^{-4})	+217	+	+

Competition for the electrons of $FMNH_2$ between the light reaction and a redox substance of suitable potential could be expected to produce the phenomenon of reversible inhibition of bioluminescence. This hypothesis was tested for methylene blue as the alternate carrier, an experimental design outlined by Ackermann and Potter (*8*) being used. Bioluminescence was measured quantitatively in a special photometer, which has been described previously (*6*).

Curve A in Fig. 1 shows that the light production in appropriately diluted suspensions of *A. fischeri* was directly proportional to the concentration of bacteria in suspension. The addition to these suspensions of methylene blue to a molar concentration of 1.6×10^{-5} resulted in approximately 90 percent inhibition of luminescence, regardless of the bacterial concentration (curve B, Fig. 1). This type of response is indicative of reversible inhibition (*8*).

Reduction of reversible redox indicators could well be brought about by tapping the electron flow of the main respiratory pathway of the bacteria; however the parallelism between inhibition of luminescence and reduction of dyes in this study suggests that the two phenomena may be related. While it is thought that TTC and methylene blue react with a flavin enzyme of the diaphorase type, the inhibition of bioluminescence by substances with more positive redox potentials suggests an interaction with at least one additional component of the light-producing system whose redox potential could be as high as +217 mv. It has been suggested that palmitic aldehyde may be such a cofactor (*9*), although enzymatic oxidation of aldehyde groups usually proceeds at less positive potentials.

McElroy and Green have propounded a hypothesis according to which two reduced flavin molecules are required for light production—one which combines with the aldehyde and a second one which forms an organic peroxide; oxidation of the aldehyde by the peroxide is considered to be the reaction that yields the energy for light production (*4*). The two authors have stated that peroxidation of the aldehyde would yield approximately 75 kcal/mole, which would be in excess of the 60 kcal required for the bacterial emission of light with a peak around 480 mμ.

If an additional electron carrier with a potential considerably more positive than that of FMN were involved in light production, it would be difficult to visualize how one pair of electrons might pass in one step across the potential difference of 1200 mv to produce the approximately 60 kcal/mole of quanta (Einstein) which correspond to the emitted wavelength around 480 mμ.

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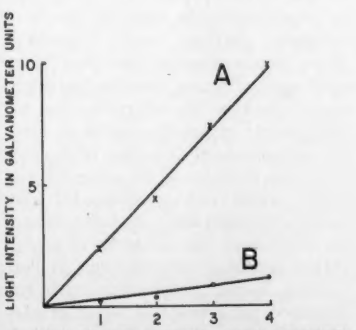

Fig. 1. Reversible inhibition of bioluminescence by methylene blue. (A) No inhibitor; (B) $(1.6 \times 10^{-5})M$ methylene blue added.

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4 October 1957

Isolation of Cytoplasmic Particles with Cytochrome Oxidase Activity from Apples

The apparent absence of cytochrome oxidase activity and the simultaneous presence of very high phenolase activity in particles isolated from plant tissues should not, without further careful scrutiny, be considered sufficient evidence for the nonparticipation of the cytochromes in the respiration of these tissues. In fact, we have found that cytochrome oxidase activity in young apples can be completely masked by the effects of low *p*H of the extracting medium.

When young Rome Beauty apples about 4 to 5 cm in diameter were homogenized in a Waring blender at 2° to 4°C

in a medium containing 0.25*M* sucrose, 0.01*M* ethylenediaminetetraacetic acid (EDTA), and 0.1*M* phosphate buffer at *p*H 7.0, no cytochrome oxidase activity was found (Fig. 1) (*1*). The homogenate rapidly turned brown during blending (30 seconds at top speed and 1 minute at a slow speed at approximately 30 v), and after centrifugation of the homogenate at 1000*g* the pellet isolated at 16,000*g* was also brown. The *p*H of the homogenate obtained by such a procedure was 6.0, and the *p*H of the particles suspended in 8 ml of 0.25*M* sucrose and 0.001*M* adenosine triphosphate (ATP) was 5.2 (*2*). The nitrogen content of 0.5 ml of the particulate suspension (derived from 200 g of tissue and 300 ml of homogenizing medium) was about 260 μ. All the phenolase activity from such a preparation was associated with the particulate fraction (Fig. 1), which showed considerable activity.

Quite a different picture emerged when the apples were homogenized in a solution containing 0.15*M* sucrose, 0.01*M* EDTA, and 0.2*M* tris-hydroxymethyl aminomethane (Tris) at *p*H 9.2. The homogenate in the Tris medium had a *p*H of 9.0 and remained green during blending. The isolated particles were green, and the particulate suspension had a *p*H of 6.2. The nitrogen content of 0.5 ml of this particulate suspension (obtained in exactly the same manner as particles isolated in sucrose, EDTA, and phosphate) was about 140 μg. Cytoplasmic particles from apples isolated in this way exhibited good cytochrome oxidase activity (Fig. 1) and showed a different distribution pattern for phenolase activity (*3*). Much of the phenolase activity in this preparation was associated with the supernatant from the high-speed centrifugation, but the cytoplasmic particles also showed significant phenolase activity (Fig. 1).

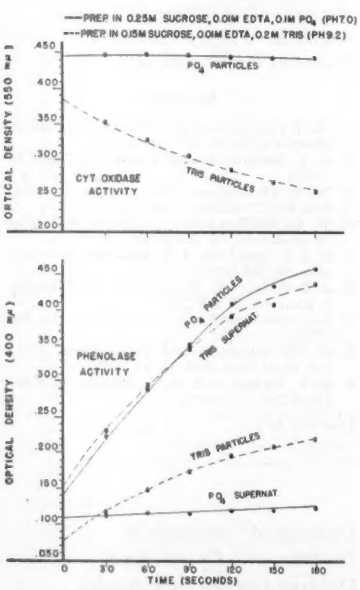

Fig. 1. (Top) Cytochrome oxidase activity of cytoplasmic particles from apples. Activity was determined by the method of Cooperstein and Lazarow (*1*). The crude enzyme used in the assay consisted of 0.03 ml of the particulate suspension. (Bottom) Phenolase activity of cytoplasmic particles and supernatant from apples. Activity was determined by the method of Ponting and Joslyn (*3*) with modifications as follows: 2.99 or 2.90 ml of 0.05*M* catechol in acetate buffer at *p*H 5.0 was the substrate, and 0.01 ml of the particulate suspension or 0.1 ml of the supernatant was added at zero time to a 3-ml cuvette. All assays were made with a Beckman DU spectrophotometer. The values shown are averages of three experiments.

Three possible explanations for the altered pattern of phenolase activity and for the considerable difference in the nitrogen content of the particulates isolated at the two *p*H levels may be proposed. If it is assumed that phenolase is localized on the cytoplasmic particles *in vivo*, then the enzyme may be removed from the particles by solubilization at *p*H 9.2 but not at *p*H 7.0. A second possibility is that phenolase, present in both soluble and particulate fractions, is agglutinated when apple cells are ruptured in a homogenizing medium at a *p*H of 7.0, but not at the higher *p*H. The third possibility is that phenolase is preferentially adsorbed on the particles in an extracting medium at *p*H 7.0, but that there is no adsorption at *p*H 9.2. Ponting and Joslyn (*3*) found that apple phenolase tightly adsorbed on particles could be solubilized by using a 2 percent sodium carbonate solution of *p*H 11.3. Hagen and Jones (*4*) observed a similar *p*H effect on catalase activity in chloroplast particles and in supernatant fractions from wheat leaves.

Hackney (*5*) reported no cytochrome *c* in apples and concluded that cytochrome oxidase is absent from them. Webster (*6*) could not demonstrate the occurrence of cytochrome oxidase in apple tissue and failed to show a reversal of carbon monoxide inhibition. This report demonstrates cytochrome oxidase in apple particulates by spectrophotometric methods. There are two additional reports (*7*) in which cytochrome oxidase activity was demonstrated by manometric techniques in tissue slices and in cell-free extracts from apples. In our work with apples it appeared that use of homogenizing medium of high *p*H (9.2) is necessary for isolating particulates in which cytochrome oxidase activity can be demonstrated. The cells of the apple fruit have a sap *p*H of 3.5 (*8*) and contain a very active phenolase system (*9*). It seems necessary to counteract the acidity and remove most of the phenolase from the particulates before good cytochrome oxidase activity appears. Since the *p*H of the cell sap of many fruit tissues is very low, and since these tissues usually show high phenolase activity, it may be necessary to use homogenizing media of high *p*H values to isolate cytoplasmic particles that show cytochrome oxidase activity (*10*).

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4 October 1957

Alteration of Clay Minerals by Digestive Processes of Marine Organisms

The possible importance of the highly acid chemical environment in animal stomachs upon clay minerals suspended in marine water was brought to our attention by two papers (*1*) which were concerned primarily with the effects of inorganic particles upon organisms. These papers demonstrated that many plankton-feeding and ooze-feeding fishes, clams, plankton animals, and other organisms are apparently unable to separate organic food particles from inorganic clay particles of the same size and cannot avoid passing suspended clay material through their digestive organs. An analysis of the stomachs of mullet (*Mugil cephalus*) from the surf at Port Aransas, Texas, showed that each fish contained an average of 1.6 g of clay within its stomach and intestine. Similar clay-filled guts were observed in menhaden, shrimp, and anchovies. That changes in the clay mineral composition of sediments being brought to the bays

and estuaries of the Texas Gulf Coast occur has been shown by Grim and Johns (*2*). This investigation (*3*) is concerned with the part played by marine organisms in bringing about these changes.

The ultimate purpose of this study is (i) to determine the amount and type of change in various clay minerals caused by representative organisms and (ii) to compare these changes in the digestive tracts with changes brought about by exposure of clays to sea water alone.

Representatives of Mollusca, Echinodermata, the filter feeding fish, and several planktonic organisms have been used for this work, to date. All organisms were placed in aquaria of filtered sea water and starved, to rid them of any previously contained clay matter. Then the organisms were transferred to fresh tanks of filtered water into which food—either algae (*Platymonas*) or yeast—and the clay mineral to be studied were introduced. Controls were set up in which these same minerals were exposed to filtered sea water of the same salinity, without the animals. Only clay material of less than 2 μ in size was used, to insure similarity between clay particle size and food particle size and to avoid contamination from other minerals which generally occur in larger grain size.

Fecal matter was carefully collected, as being representative of material which had actually passed through the animal. The fecal material was washed free of salt and treated with 30 percent hydrogen peroxide to rid it of organic matter. X-ray diffraction, differential thermal analysis, and electron microscopy were used to characterize the minerals before treatment, after passage through organisms, and in controls. All samples used for diffraction work were conditioned at 50 percent relative humidity at 20°C, except during special treatments. Data were obtained both from x-ray diffraction photographs and from spectrometer recordings.

The major part of the work has been done with a soil containing randomly interlayered clays, a bentonite, and kaolinite. Kaolinite does not appear to be affected by either sea water or organisms. However, the interlayered minerals and the bentonite are readily subject to modification and exhibit appreciable changes both within the control samples and in animal tanks after periods of only 24 hours.

Representative data are presented in Table 1. These experiments tend to indicate a build-up of magnesium within the exchangeable cation positions upon exposure to sea water alone which, for the bentonitic montmorillonite, produces no change in 001 peak position during the 24-hour period. A similar build-up of magnesium within the soil clay, however, tended to prop the structure open to 15.5 A, thus blanketing out the effect of the poorly developed brucite interlayer already present. Heat treatment of this organization then collapsed the mica layers containing magnesium but only gradually affected the portions containing interlayered islands of brucite.

Table 1. Effect of organisms on x-ray characteristics of ingested clays; (001) *d* values from orientated aggregates of clays before treatment, in controls, and after passage through organisms. All work was done with Cu*Kx* radiation.

Treatment	(001) *d* values (A)								
	24-hour exposure					5-day exposure			
	Original	Control	Oyster	Clam	Mullet	Control	Oyster	Clam	Mullet
					Soil montmorillonite				
50% relative humidity	14.7	15.5	12.6	12.1	14.7	14.9	12.9	12.3	14.3
Glycol	17.0	17.0	17.0	17.0	17.0	17.0	17.0	17.0	17.0
300°C	12.6	14.5	12.6	11.8	12.5	14.5	12.9	12.1	12.6
					Bentonitic montmorillonite				
50% relative humidity	15.5	15.2	14.7	14.3	14.3	14.7	12.9	12.9	12.6
Glycol	17.0	17.0	17.0	17.0	17.0	17.0	17.0	17.0	17.0
300°C	10.0	10.0	10.0	10.0	10.0	12.8	10.0	10.0	10.0
					Kaolinite				
50% relative humidity	7.3	7.3	7.3	7.3	7.3	7.3	7.3	7.3	7.3

The same bentonitic montmorillonite, when exposed to sea water for a period of 5 days, appears to begin the development of a brucite configuration around the magnesium ions in the manner described by Grim and Johns (*2*); this results in a shifting of the 001 peak position from 15.5 A to 14.7 A. This new structure expanded completely upon glycolation but only gradually collapsed through 500°C. Exposure of the soil clay to the control water for the same length of time resulted only in an enhancement and slight shift in position of the original 14.7 A peak.

Exposure of these same clays to the organisms for like periods of time always resulted in a large shift in the 001 peak position toward 10.0 A. Although glycol apparently produces complete expansion, we suggest that potassium in the form of an exchange cation is being taken up by all samples upon exposure to the animals. It also seems likely that, within the soil clay, the brucite islands are being at least partially destroyed by the stomach acidity of the organisms. A relative decrease in influence from the 14.0 A component was noted in the soil clay, and no development of such a component could be observed within the bentonitic montmorillonite.

Thus, the two processes of change may be working independently of one another. According to this interpretation, clays of the 2:1 layer type, upon exposure to sea water, begin the uptake of magnesium within the exchangeable cation positions almost immediately and gradually begin the development of a brucite configuration between mica layers. Similar clays which pass through organisms, however, suffer a loss of such brucite and build up potassium as an interlayered cation. Whether or not this potassium begins to exert any more influence upon the structure with time and constant reexposure may be determined in future work.

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30 September 1957

Book Reviews

No Frontier to Learning. The Mexican student in the United States. Ralph L. Beals and Norman D. Humphrey. University of Minnesota Press, Minneapolis, 1957. xi + 148 pp. $3.25.

This is fascinating reading. Ten Mexican students at the University of California (Los Angeles) were intensively interviewed and tested (from 4 to 20 hours) about such things as their motivations in coming to the United States, their attitudes toward American culture before, during, and after their period of study, their problems of linguistic, academic, and social adjustment, and their beliefs about the value of their foreign study upon returning home. An additional 42 Mexican students supplied supplementary information. Twenty-six Mexican students who had returned were interviewed in Mexico. The methodological approach is anthropological; a senior author with over 25 years, and a junior author with 15 years, of first-hand acquaintance with Mexico and Mexican culture worked intensively with relatively few subjects. I believe that the conclusions reached are accurate and that extensive use of questionnaires among a larger sample would have produced few differences. Nevertheless, a follow-up in other parts of the United States would be of interest: only eight of the 52 students resided outside of California.

Some of the findings are as follows: most of the students come from middle- and upper-class urban backgrounds; all reflect considerable anti-American feeling, in considerable measure because of historical reasons rather than personal experiences; the desire of many to study in colleges in the United States is motivated by a sense of social obligation and the wish to do something for Mexico; most students would have preferred to study in Europe; most students adjust to United States culture more readily than does the average foreign student. The students' attitudes became modified in the direction of greater egalitarianism, increased cooperativeness, greater open-mindedness, and more feelings of social responsibility. Many shifted from a position of hostile criticism to friendly criticism, but all continue to believe in the basic superiority of Mexican life goals, "with their emphasis on spiritual and humanistic values." Discouragingly, most returned students find a cultural climate in which they often must be overtly anti-American in order to be accepted and to get ahead in their fields. The authors point out that if the major objective of exchange programs is to secure wholehearted approval of the United States by foreign students, the Mexican experience indicates that this goal is not reached. In broader perspective, the reader is left with the conviction that both Mexico and the United States profit from the presence of the Mexican student in this country.

Would it be in order to suggest that a companion study, "The American Student in Mexico," would be equally desirable?

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Proceedings of the XIV International Congress of Zoology. Copenhagen, 5–12 Aug. 1953. Danish Science Press, Copenhagen, 1956. 567 pp. Illus. Paper, Kr. 100.

The proceedings of the 14th International Congress of Zoology, held in Copenhagen in 1953, were published last year but only recently received wide distribution. Delegates from 34 countries and three international institutions were present. It may be of interest to note that 501 members took part, in contrast to the 707 who participated in 1927 in Budapest, where the tenth congress was held.

A casual reader may well ask: What goes on at an international congress of zoology? By way of answer, this volume clearly says, "All sorts of new and old biological ideas are discussed, and many viewpoints are interrelated." In addition to the general meetings, which covered subjects such as fauna protection, improving zoological nomenclature, growth of marine populations, paleoecology and adaptation, and thinking without words (precursors to language), and films on the Galathea Expedition, the history of the eel, and the flight of insects, there were 16 sectional meetings, at each of which some 10 to 25 papers were presented. Some sample groupings are paleozoology, evolution, cytology, morphogenesis, psychology, comparative physiology, serology, parasitology, nematology, hydrobiology, and anthropological systematics.

It would be all too easy to suggest that this eclectic collection is without great permanent value, or that zoological congresses are scientifically passé. A brilliant defense of general meetings is found in Professor Spärck's presidential address of welcome. He faces the problem courageously: "Some people are of the opinion that in zoology specialization has now developed so far, that we should stop having general congresses of zoology, and only have congresses of specialists. . . . It is important not to forget that the animal Kingdom is a part of nature, that a zoologist is a naturalist, that the problems of cytology, biochemistry, physiology concern living animals, reacting as a whole organism against ecological factors and other organisms. . . . Apart from the lectures, excursions, and personal contacts, the congresses are a very important factor in international cooperation. They are a counterweight against isolation, which is so dangerous to the progress of Science."

Brief mention of a few of the subjects discussed and of their proponents may illustrate the true value of this triennial meeting: the giant mitochondria of insect muscles (C. Williams); representative Mediterranean fauna in the Caspian Sea (L. Zenkevitch); radiation effects on regeneration of invertebrates (E. Wolff); nucleolar extrusion in amebae (D. L. Ray); olfactory sense of dogs in relation to fatty acids (T. Uchida); proposals for international agreement on normal stages in vertebrate embryology (E. Witschi); control of tube-worm growths in a British tidal harbor lake (N. Tebble); tumorgenesis in invertebrates (J. Wautier); the biology of Atlantic seals (a new color film, presented by L. H. Matthews); toxonomic characters of Old World monkeys (M. Friant); specificity of food plants for insects (G. Fraenkel); and ways of stabilizing zoological nomenclature (C. L. Hubbs). Unfortunately, many papers were not up to the standards of these.

The congress was convened under the auspices of the Danish Government and the University of Copenhagen. Financial aid was received from the Carlsberg and Tuborg brewing industries. The volume contains a few figures and an index and has a paper cover of good quality. One may well ask why some 13 authors could not take the trouble to submit abstracts. Biologists generally, and zoologists in particular, should do some

serious thinking about the subject matter of this volume. Unless general congresses are more highly appreciated, Professor Spärck's remarks may have been made in vain.

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Through Alchemy to Chemistry. A procession of ideas and personalities. John Read. Bell, London, 1957 (order from Macmillan, New York). xvii + 206 pp. Illus. $3.75.

John Read has added to his books on organic chemistry and alchemy a new and particularly delightful survey of historical developments from alchemy to present-day chemistry. Although only 206 pages long, including three of glossary and ten of index, this is a very readable introduction to the history of chemistry. The emphasis rests on alchemy, the discussion of which comprises about one-half of the volume. Although the story of Paracelsus does not begin until the seventh of the ten chapters, some main lines of "modern" chemistry are clearly and effectively drawn. However, Read confines this part of the picture too severely and shows only something of the chemicals, nothing about chemical reactions.

The short biographies are masterly, informative, and entirely reliable. For example, Lavoisier's work in "agriculture and many other national matters" is not omitted, as it sometimes is even in larger histories of chemistry.

The text is enlivened by 50 illustrations, showing portraits, equipment, and working scenes, including the "tailpiece" from J. B. Porta's book of 1608 on distillation.

This is the sort of book which makes a welcome gift for students of any kind and age, providing relaxation and stimulation.

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Die Evolution der Organismen. Part 4; part 5. Gerhard Heberer, Ed. Fischer, Stuttgart, Germany, 1955; 1957. 143 pp.; 252 pp.

The new edition of the great German handbook of evolution is nearing completion with publication of the five chapters of the two latest installments. A sixth and final part is in preparation.

Part 4 begins with a discussion by Franz Schwanitz of the origin of cultivated plants. In his classic *Origin of Species,* Darwin very effectively used the origin of cultivated plants and domestic animals to substantiate the all-importance of selection. A review of the results of plant and animal breeding is therefore a legitimate component of a modern review of the theory of evolution. In this survey of plant breeding, Schwanitz finds confirmation for Darwin's contention that selection can create almost anything it wants. Varieties of plants have been produced with large size, high productivity, the loss of certain undesirable characteristics, high or low variability, or other, sometimes highly improbable, attributes. The changes occur amazingly fast if large enough numbers of individuals are available for selection. Here we have experimental evolution in its truest form. Schwanitz presents the recent results in this field and attempts to determine the respective contribution of mutation (including chromosomal mutations and polyploidy), hybridization, and other factors. It is a competent treatment of the established knowledge, with an apparently deliberate avoidance of some of the more controversial and more speculative aspects of the field.

The corresponding chapter on animals, by Herre, brings together much of the exceedingly scattered literature on the domestication of animals. Earlier claims for a hybrid origin of domestic animals have not been confirmed; indeed, conclusions drawn from the detailed analyses of fossils associated with prehistoric man are in complete opposition to such an interpretation. There is great parallelism in the phenomena of domestication, not only when different species of wild animals are brought into domestication but also when the same species is repeatedly domesticated in different areas. Herre particularly emphasizes the effects of domestication on various organ systems, such as the endocrine, and on body proportions, somewhat along the lines of Stockard's work.

Much in current accounts on domestication is still descriptive and anecdotal. Indeed one has the feeling that the biology of domestication is a potential gold mine, of which only the surface has so far been scratched. The most recent findings of population genetics, for instance, have hardly been applied to an interpretation of the phenomena of domestication. The breakdown of developmental and genetic homeostasis in the wake of one-sided selection is, as Lerner has pointed out, undoubtedly one of the main causes of the domestication phenomena.

The various theories concerning the origin of new phyletic types are discussed by Heberer in a chapter (the first of part 5) that is particularly valuable from the standpoint of methodology. Heberer shows that the concept of gradual evolution is favored by all the available evidence and that the hypothesis of the origin of new types through saltations (macromutations) is based on a misunderstanding of known genetic mechanisms. The phenomena of mosaic evolution (particularly among so-called "missing links") and the invariable coincidence of gaps in phyletic series with gaps in the fossil record greatly strengthen the theory of gradual evolution. The documentation of this thesis by Heberer is broad and convincing, and it contains much that is new and original.

Von Krogh gives an orthodox summary of the anatomical similarities and differences between man and the other primates. The fossil history of man is presented by Gieseler in great detail (159 pages) and with abundant illustrations. As a summary of the literature this account is very useful. Unfortunately there is little attempt at synthesis. On the whole, *Pithecanthropus, Sinanthropus,* Neanderthal, and so on, are still presented as so many "types" of fossil hominids, without any real biological interpretation. As long as only a few scattered remains of fossil man existed, no other course was open. The time has now come, however, for bold hypotheses aiming to make sense of the diversity of remains of fossil man. Such hypotheses can be made only through analogy with the variation, in space and time, of other species of mammals.

The work continues to be very attractively illustrated. It contains an unusual amount of material that would be suitable as illustration for lectures and in the classroom.

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Electron Microscopy. Proceedings of the Stockholm Conference, September 1956. F. S. Sjostrand and J. Rhodin, Eds. Academic Press, New York, 1957. xi + 355 pp. Illus. $17.50.

The papers presented at the Electron Microscopy Conference held in Stockholm in September 1956 are divided into 14 sections: "Instrumentation," "Electron Optics," "Electron-Specimen Interaction," "High Resolution Electron Microscopy and Electron Diffraction," "Specimen Preparation Techniques in Biology and Science," "Cell Ultrastructure, General," "Nerve Cells and Receptors," "Muscle and Other Contractile Elements," "Collagen, Cartilage, Bone," "Pathology," "Microbiology," "Botany," "Paper and Textile Research," and

"Metallography and Other Industrial Applications."

The book is well organized, in view of the numerous fields of interest, and the reproduction of the photographs is excellent. The reader will get more out of this book if he has command of German, since about one-third of the articles are in that language.

The papers dealing with reflection and scanning electron microscopes indicate the interest and efforts being expended on development of instruments wherein thin specimens are not necessary. The techniques of sample preparation as described in some of the papers should aid workers who are trying to develop similar techniques for their own work.

The efforts to obtain high resolution have been successful to the point where 10 angstroms has been gained; however, it is apparent that such high resolution is obtained only with ideal samples and with ideal instrument performance; the normal resolution remains in the range of 25 to 50 angstroms.

The number of papers in the interesting and important field of metallurgy is quite small. Two very interesting and important papers on the study of dislocations in thin foils were presented. The techniques presented in such papers should promote greater interest in the study of the dislocation theory by means of the electron microscope.

This publication illustrates the extremely wide scope of electron microscopy. While the book itself cannot be recommended as a reference book, it embraces work from leading scientists in a very large number of fields, both here and abroad.

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Purdue University

An Encyclopedia of Annual and Biennial Garden Plants. Charles O. Booth. Faber & Faber, London, 1957 (order from Macmillan, New York). 488 pp. Illus. $12.50.

The zest with which American publishers view erudite British garden books appears to be irrepressible. Obviously expert in treatment, and somewhat overpowering in content, this British import deals, with painstaking care, with hundreds of annuals and biennials that most American gardeners will never see. To give an example—the author treats of 39 annual or biennial species of *Campanula*, and of these only about half a dozen are likely to be in cultivation here.

The book, then, is patently for the experts, and it would be difficult to find a better one. All experiment-station workers in horticulture, writers, and editors will find in it—what is nowhere else so well done—careful, precise definitions of over 1400 species and varieties, facts about their culture and care and about their pests, and even their chromosome numbers.

About two-thirds of the book is in the nature of an encyclopedia; the rest consists of introductory matter and appendixes, such as a glossary of terms, a table of specific equivalents, an index of common names, a table of chromosome numbers, an index of pests, and a list of seedsmen in overseas (non-British) countries. (Many of those mentioned as being seedsmen in the United States are actually nurserymen and sell no seeds, and one of the seedsmen has been out of business for four years.) There is also a list of pest-control-product manufacturers—all British, hence the list is useless here. In spite of these handicaps, this is a highly recommended book, well worth its rather steep price but obviously not for browsing by amateurs.

NORMAN TAYLOR
Elmwood, Princess Anne, Maryland

The Chemistry of Organometallic Compounds. Eugene G. Rochow, Dallas T. Hurd, and Richard N. Lewis. Wiley, New York; Chapman & Hall, London, 1957. vi + 344 pp. $8.50.

Teachers of graduate courses in organometallic chemistry will be indebted to the authors for a book which presents a systematic approach to the subject and should serve admirably as a textbook. The book is obviously not intended to be a comprehensive or detailed treatment of organometallic phenomena, but rather a systematic survey of the field, with generous lists of literatur references for the reader whose interest has been stimulated. Those engaged in organometallic research will find the many tables useful, although the descriptive portions are too limited in detail to be of much reference value in any specialized area.

The systematic approach is developed from a general consideration of the carbon-metal bond and the types of bonds which characterize the groupings of organometallic compounds. The authors judiciously have not attempted a rigid classification into types and subtypes, preferring rather to discuss groupings defined by the periodic table of the elements. A separate chapter, which should appeal to students, is devoted to a general survey of preparative methods.

Readers who are expert in specific areas covered by the book may take issue with certain explanations or observations in their field of specialization. This does not detract unduly from the value of the book as a systematic approach to organometallic chemistry.

The authors are to be complimented on a very timely introduction to a subject which has aroused considerable interest in both industrial and academic circles.

THOMAS P. WHALEY
Ethyl Corporation,
Baton Rouge, Louisiana

Radiation Shielding. B. T. Price, C. C. Horton, and K. T. Spinney. Pergamon Press, London and New York, 1957. ix + 350 pp. Illus. $10.

This book, volume 2 of Division X (*Reactor Design Physics*) of the *International Series of Monographs on Nuclear Energy*, deals with the problems of shielding modern nuclear equipment such as reactors, betatrons, synchrotrons, and linear accelerators. During nearly ten years of reactor construction, shielding was handled primarily on an empirical basis, resulting in unnecessarily heavy and expensive designs. With modern developments, such as mobile reactors operating at sea and commercial exploitation of nuclear power on land, the emphasis in radiation shielding has shifted from the purely empirical phase into the area of theoretical understanding. The book puts the emphasis on the physical principles of shielding after presenting, in the first chapter, an excellent survey of the background cause of all our shielding problems and needs: the biological hazards of nuclear radiations. The discussion is confined to the requirements of the aforementioned equipment, leaving very high-energy accelerating machines for a later study. The individual chapters deal with the attenuation of gamma rays and high-energy electrons, with neutron physics, and with neutron attenuation in thick shields. A special chapter gives often-used mathematical formulae for different source-sizes and shapes.

The last chapter presents applications of the physical principles to practical problems, emphasizing the often complicated task of designing the proper shield for very powerful equipment. A great number of valuable material-constants and figures make this chapter of special value to the engineer and physicist working in this challenging field. At a time when nuclear power, from its role at sea, on land, and in the air, has stepped over the threshold into outer space, this book will be very welcome.

A. T. KREBS
U.S. Army Medical Research Laboratory

Ultrastructure and Cellular Chemistry of Neural Tissue. vol. II of *Progress in Neurobiology*. Heinrich Waelsch, Ed. Hoeber-Harper, New York, 1957. xv + 249 pp. Illus. $7.50.

This volume contains the proceedings of the second of the symposia organized by Saul Korey and John Nurnberger. The organizers deserve commendation for these discussions. Themselves able clinicians as well as scientists, their policy of holding these seminars coincident with clinical meetings serves as an important adjuvant to the interpenetration of the basic and clinical sciences. The present volume has no unifying theme but is, rather, a sampling of the entire field. Heinrich Waelsch's introduction succinctly justifies the existence of neurochemistry as a field in its own right and pleads for basic neurological research unhampered by demands for practical results.

What is so impressive is the multiplication of techniques and the correlations between so many fields. Appropriately, the volume opens with papers, by Robertson and Palay, stressing the relationships of chemical composition and function to ultrastructure. Among the remaining papers the dominant spirit is that of the replacement of the older chemistry and metabolism of nervous tissue as a whole by the precise localization of metabolic processes and the characterization of single components. Lowry's study of enzymes in single neurons, Geiger's technique for the tissue culture of cortical cells, Nurnberger and Gordon's differential counting of neuronal and non-neuronal nuclei, Loeser's method of scanning of cells stained with fluorescent dyes illustrate this trend. It is also seen in the four papers on the histochemical localization of enzymes in individual cells (Koelle, Gomori) and in those on the layers of cerebral cortex (Pope, Hess, and Allen) and cerebellum (Robbins, Smith, and Jen). The other contributions in the volume study biochemical changes in relation to development (Keup), neuropathology (Feigin, Wolf, and Carter; Pope), and drug actions (Bain, Berger). Lajtha and Furst discuss the blood-brain barrier and protein turnover.

If the volume has a defect it is in the omission of any discussion of the control of ionic movements in nerve, so central to modern electrophysiology and an important frontier of neurochemistry. It should also be mentioned that the date on which this symposium was held is kept a very dark secret throughout the volume.

NORMAN GESCHWIND
Massachusetts Institute of Technology

New Books

Atomic Energy in Medicine. K. E. Halnan. Philosophical Library, New York, 1957. 167 pp. $6.

Buildings for Industry. An architectural record book. F. W. Dodge Corp., 1957. 309 pp. $12.50.

Physics and Chemistry of the Earth. vol. 2. L. H. Ahrens, Frank Press, Kalervo Rankama, S. K. Runcorn, Eds. Pergamon Press, New York and London, 1957. 267 pp. $10.

Autonomic Imbalance and the Hypothalamus. Implications for physiology, medicine, psychology, and neuropsychiatry. Ernst Gellhorn. University of Minnesota Press, Minneapolis, 1957. 314 pp. $8.50.

Religion, Philosophy, and Science. An introduction to logical positivism. Burnham P. Beckwith. Philosophical Library, New York, 1957. 251 pp. $3.75.

Intention. G. E. M. Anscombe. Cornell University Press, Ithaca, N.Y., 1957. 104 pp. $1.50.

Of Things Bi-Illogical. Collected by Bernal R. Weimer. Bethany Press, St. Louis, 1957. 79 pp.

Circles. D. Pedoe. Pergamon Press, New York and London, 1957. 88 pp. $3.75.

Fundamentals of Hydro- and Aeromechanics. Based on lectures of L. Prandtl. O. G. Tietjens. Translated by L. Rosenhead. Dover, New York, 1957. 286 pp. $1.85.

Hemophilia and Hemophilioid Diseases. International Symposium. Kenneth M. Brinkhous, Ed. University of North Carolina Press, Chapel Hill, 1957. 289 pp. $7.50.

Physik und Technik der Atomreaktoren. Ferdinand Cap. Springer, Vienna, Austria, 1957. 516 pp. $15.25.

Microscopic Anatomy of the Temporal Bone. A photographic survey of serial sections of the temporal bone cut in the three routine planes of sectioning human specimens. Dorothy Wolff, Richard J. Bellucci, Andrew A. Eggston. Williams & Wilkins, Baltimore, 1957. 422 pp. $12.50.

Insect Flight. J. W. S. Pringle. Cambridge University Press, London, 1957. 141 pp. $3.

Textile Chemicals and Auxiliaries. With special reference to surfacants and finishes. Henry C. Speel and E. W. K. Schwarz. Reinhold, New York; Chapman & Hall, London, 1957. 551 pp. $13.50.

Non-Group Enrollment for Health Insurance. A study of administrative approaches of Blue Cross plans. Sol Levine, Odin W. Anderson, Gerald Gordon. Harvard University Press, Cambridge, Mass., 1957. 182 pp. $5.

The Immense Journey. Loren Eiseley. Random House, New York, 1957. 210 pp. $3.50.

How to Do an Experiment. Philip Goldstein. Harcourt, Brace, New York, 1957. 192 pp. $2.60.

Advances in Enzymology. And related subjects of biochemistry. vol. XIX. F. F. Nord, Ed. Interscience, New York, 1957. 462 pp. $9.85.

Advances in Veterinary Science. vol. III. C. A. Brandly and E. L. Jungherr, Eds. Academic Press, New York, 1957. 590 pp. $13.

Crop Production in the South. Glenn C. Klingman. Wiley, New York, 1957. 424 pp. $4.95.

Man-Made Moons. The earth satellites and what they will tell us. Irving Adler. Day, New York, 1957. 128 pp. $2.95.

The Pigeon. Wendell Mitchell Levi. Levi Publishing, Sumter, S.C., ed. 2, 1957. 694 pp. $20.

The Planet Earth. D. R. Bates. Pergamon Press, London and New York, 1957. 317 pp. 35s.

Yeasts. K. Arima, W. J. Nickerson, M. Pyke, H. Schanderl, A. S. Schultz, A. C. Thaysen, R. S. W. Thorne. W. Roman, Ed. Junk, The Hague, 1957. 246 pp.

Climatology. Treated mainly in relation to distribution in time and place. W. G. Kendrew. Oxford University Press, London, ed. 2, 1957. 415 pp. $6.75.

The Analytical Uses of Ethylenediamine Tetraacetic Acid. Frank J. Welcher. Van Nostrand, Princeton, N.J., 1957. 380 pp. $8.50.

Chemotherapy and the Central Nervous System. Henry McIlwain. Little, Brown, Boston, 1957. 336 pp. $10.

Miscellaneous Publications

(Inquiries concerning these publications should be addressed, not to Science, *but to the publisher or agency sponsoring the publication.)*

Changes in the Fauna of Ontario. F. A. Urquhart. University of Toronto Press for Royal Ontario Museum, Toronto 5, Canada, 1957. 75 pp. $1.50.

Washington's Potowmack Canal Project at Great Falls. A Great Falls Park and Nature Preserve. A proposal for the preservation of a natural area and the restoration of a historic site as a lasting monument to George Washington at the Great Falls of the Potomac in Virginia. C. J. S. Durham. Nature Conservancy, Wildlife Management Institute and Fairfax County Park Authority, Washington, D.C. and Fairfax County, Va., 1957. 44 pp.

Pelagic Polychaetes of the Pacific Ocean. Bulletin of the Scripps Institution of Oceanography, vol. 7, No. 2. R. Phillips Dales. University of California Press, Berkeley, 1957. 70 pp. $3.

American Society for Artificial Internal Organs, Transactions. vol. III. Chicago, 14–15 Apr. 1957. For the Society by Georgetown University Printing Dept., Washington, 1957 (available from G. E. Schreiner, Georgetown University Medical Center, Washington, D.C.). 96 pp.

1957 National Conference on Instrumental Methods of Analysis. ISA Proceedings. Instruments Society of America, Pittsburgh 22, 1957. 25 papers. $7.50.

Bengal Immunity Research Institute, Five-Year Report (Jan. 1951–Dec. 1955). The Institute, 39, Lower Circular Road, Calcutta 16, India, 1957. 35 pp.

Bibliography of Scientific Publications of South and South East Asia. vol. 1, 1955. UNESCO Science Cooperation Offices for South and South East Asia. INSDOC, National Physical Laboratory, New Delhi, India, 1957. 188 pp.

Meetings and Societies

Cellular Mechanisms of Physiological Adaptation

The twelfth annual meeting of the Society of General Physiologists was held at the Marine Biological Laboratory, Woods Hole, Massachusetts, 4–6 September 1957. On the first day contributed papers were presented, and the annual business meeting was held.

The next two days were devoted to a symposium, organized by C. Ladd Prosser, on physiological adaptation. Three previous symposia sponsored by the society have been published by the American Physiological Society. Similar arrangements have been made to publish the 1957 symposium.

An important objective of the Society of General Physiologists is to focus the attention of animal, plant, and microbial physiologists on functional properties common to all organisms. The 1957 symposium of the society dealt with cellular mechanisms of physiological adaptation. Animal physiologists have studied adaptation to environmental change largely in terms of responses of the total organism and specific function systems; effects of physical factors in the environment—temperature, oxygen, light—have been much considered. Plant physiologists emphasize selection of genetic varieties as the principle mode of adaptation in plants. Bacterial physiologists have noted genetic adaptation to physical factors but have concentrated on adaptations to changed nutrients and have used enzyme induction as a tool for studying protein synthesis.

In this symposium there was an attempt to bring together the divergent approaches of animal, plant, and microbial physiologists on adaptation. The first four papers dealt with adaptive responses of animals. Kenneth C. Fisher (University of Toronto) presented research by himself and others from the Toronto group on adaptation to temperature and to reduced oxygen, particularly in fish and rodents. Acclimatization occurs as a result of adaptive changes—for example, in locomotor activity and in oxygen consumption—made in response to exposure of the animal to lethal temperatures or to any temperature selected for study. Different adaptive changes are made at different rates during acclimatization. Seasonal acclimation in nature is often not equivalent to laboratory acclimatization.

Herbert Precht (Kiel University) considered enzymatic mechanisms of temperature adaptation in fish and in yeast. Complete acclimatization means that a rate function is the same at a temperature of acclimatization (t_2) as at a previous temperature (t_1); some oxidative enzymes show this, many show slight increase in activity after acclimatization (to a raised temperature), and a few show a decrease. The role of the endocrines in mediating the cellular changes in fish appears to be complex.

The time course of response of poikilotherms to temperature change was discussed by J. W. R. Grainger (Hull University). There is an initial overshoot of most responses (both in animals and in yeast) followed by a steady state which is the usually measured rate response to temperature change. Acclimatization is a much later compensatory change.

Otto Kinne (Kiel University) illustrated adaptation of euryhaline animals to different salinities by showing extensive changes in cell morphology in *Cordylophora* kept at high and low salinities. In this coelenterate as well as in euryhaline gammarids, an interrelation exists between adaptation to salinity and temperature.

Biochemical adaptation in animals was discussed by W. E. Knox (Harvard Medical School); the tryptophan peroxidase-oxidase system of mammalian liver was used as an example. An increase in tryptophan peroxidase-oxidase can be elicited by the adrenal cortex; the enzyme can also be induced by injection of tryptophan, even after adrenalectomy.

F. W. Went (California Institute of Technology) pointed out the difficulties of using the same definition of adaptation for plants as for animals. Strains of plant species living in different environments show striking physiological adaptations. Plants also show direct growth responses to temperature and photoperiod.

The last three papers dealt with adaptation in bacteria. Aaron Novick (University of Chicago) described induction of β-galactosidase and of the permeability-promoting permease in *Escherichia coli*. The sugar substrate or related compounds can induce permease in cells which then are "preinduced" in that they will more readily form β-galactosidase. This adaptation by a change in permeability in certain cells, although environmentally induced, behaves as if it were genetic.

Werner Maas (New York University) showed how final products of a metabolic chain may have a feedback effect on production of intermediates. For example, exogenous arginine inhibits the synthesis of several enzymes involved in arginine synthesis.

Regulation of synthesis of inducible and of constitutive enzymes in bacteria was discussed by Boris Magasanik (Harvard Medical School). Energy-liberating systems interact with enzymes of protein synthesis; a substance such as glucose may inhibit formation of an enzyme such as histidase according to the nature of limiting conditions.

In a summary statement C. L. Prosser (University of Illinois) suggested that the apparent similarity between animals and bacteria (and yeast) in adaptation by enzyme induction may be the result of chemical integration throughout each individual organism, while cells of plants are more independent, and plants rely more on genetic variation for adaptation. It was also suggested that enzyme induction may operate in adaptation to physical factors such as temperature if parallel and interacting enzyme systems are differentially affected by the physical factors and if intermediate compounds accumulate and act to induce or inhibit enzyme synthesis.

It is hoped that the symposium may help animal, bacterial, and plant physiologists concerned with adaptations to the environment to recognize common problems and to apply methods which have been useful with one kind of organism to another kind.

Results of the mail balloting for officers and council were announced at the business meeting [*Science* **126**, 659 (1957)]. F. H. Johnson and K. S. Cole were elected to serve two-year terms as councilors of the Society of General Physiologists. It was reported previously [*Science* **126**, 659 (1957)] that they had been elected as representatives to the AAAS Council.

The abstracts of the contributed papers were published as a supplement in the October issue of the *Journal of Cellular and Comparative Physiology*.

C. L. Prosser
Department of Physiology, University of Illinois, Urbana
F. G. Sherman
Department of Biology, Brown University, Providence, Rhode Island

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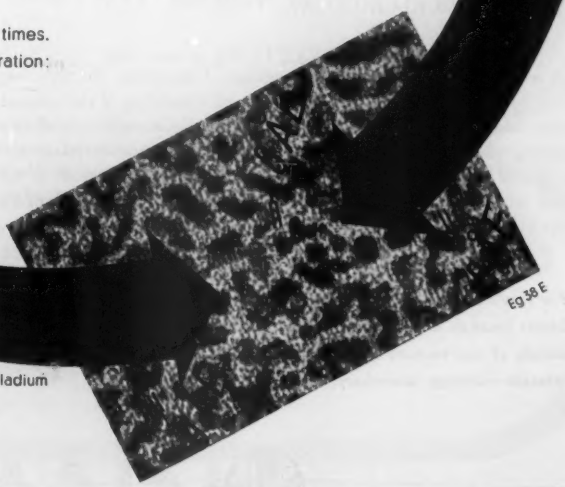

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Canadian Biological Societies

The Board of the Canadian Federation of Biological Societies was formed in Ottawa, 11 October 1957. The federation comprises four member societies—the Canadian Physiological Society, the Pharmacological Society of Canada, the Canadian Association of Anatomists, and the Canadian Biochemical Society. The federation will hold its first annual meeting in Kingston, Ontario, in June 1958. For information, write to Dr. E. H. Bensley, Montreal General Hospital, Montreal 25, Quebec.

Reticuloendothelial System

The third International Symposium on the Reticuloendothelial System will be held 28–30 August in St. Margherita Ligure, Italy. This will be a limited symposium with 30 speakers and 20 to 40 auditors. The subjects which will be covered are the reticuloendothelial system and radiation, the RES and steroids, and the RES and immunity, including protein synthesis, antibody synthesis, antigens, endotoxins, properdin, and tumors.

It is anticipated that funds will be available to defray the cost of the Americans participating in this symposium. Titles and abstracts should be submitted in duplicate *by 15 February* to Dr. John Heller, New England Institute for Medical Research, Ridgefield, Conn., or Dr. Alberto Marmont, Clinica Medica Universitaria, Genoa, Italy.

IRE National Convention

The National Convention of the Institute of Radio Engineers, Inc., will be held in New York, 24–27 March. Some 275 papers will be presented during 55 sessions. The high point of the program will be two special symposia, one on electronics in space, the other on electronic systems in industry.

The Radio Engineering Show has been expanded to accommodate approximately 850 exhibitors. The latest in electronic equipment will be on display, much of it for the first time. Further information may be obtained from the IRE, 1 E. 79 St., New York 21, N.Y.

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Forthcoming Events

February

19–21. Genetics Soc. of Canada, 3rd annual, Edmonton, Alberta, Canada. (L. P. V. Johnson, Dept. of Plant Science, Univ. of Alberta, Edmonton.)

20–21. Transistor and Solid State Circuits Conf., Philadelphia, Pa. (J. H. Milligan, Jr., Dept. of Electrical Engr., New York Univ., New York 53.)

22–25. American Educational Research Assoc., St. Louis, Mo. (F. W. Hubbard, AERA, 1201 16th St., NW, Washington 6.)

24–28. American Soc. of Civil Engineers, Chicago, Ill. (W. W. Wisely, ASCE, 33 W. 39 St., New York 18.)

March

1. Junior Solar Symposium, Tempe, Ariz. (Association for Applied Solar Energy, 3424. N. Central Ave., Phoenix, Ariz.)

1–3. National Wildlife Federation, St. Louis, Mo. (E. F. Swift, NWF, 232 Carroll St., NW, Washington 12.)

3. Wildlife Soc., annual, St. Louis, Mo. (D. L. Leedy, U.S. Fish and Wildlife Service, Washington 25.)

5–6. Gas Conditioning Conf., 7th annual, Norman, Okla. (M. L. Powers, Extension Div., Univ. of Oklahoma, Norman.)

6–8. Fundamental Cancer Research, 12th annual, Houston, Tex. (W. K. Sinclair, M. D. Anderson Hospital and Tumor Inst., Univ. of Texas, Houston 25.)

10–13. American Assoc. of Petroleum Geologists, annual, Los Angeles, Calif. (R. H. Dott, AAPG, Box 979, Tulsa 1, Okla.)

10–13. Society of Economic Paleontologists and Mineralogists, annual, Los Angeles, Calif. (R. H. Dott, Box 979, Tulsa, Okla.)

16–21. Nuclear Engineering and Sci-

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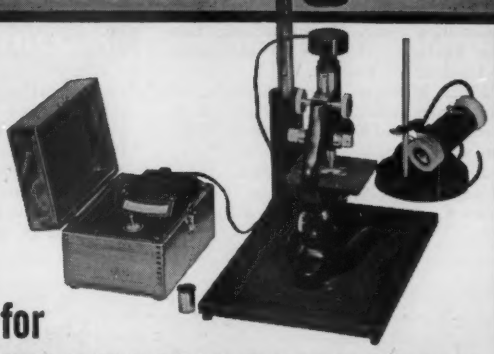

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ence Cong., Chicago, Ill. (D. I. Cooper, *Nucleonics*, 330 W. 42 St., New York.)

17–21. National Assoc. of Corrosion Engineers, 14th annual, San Francisco, Calif. (NACE, Southern Standard Bldg., Houston 2, Tex.)

18–20. Amino Acids and Peptides, Ciba Foundation symp. (by invitation), London, England. (G. E. W. Wolstenholme, 41 Portland Pl., London, W.1.)

20–22. Michigan Acad. of Science, Arts and Letters, annual, Ann Arbor. (R. F. Haugh, Dept. of English, Univ. of Michigan, Ann Arbor.)

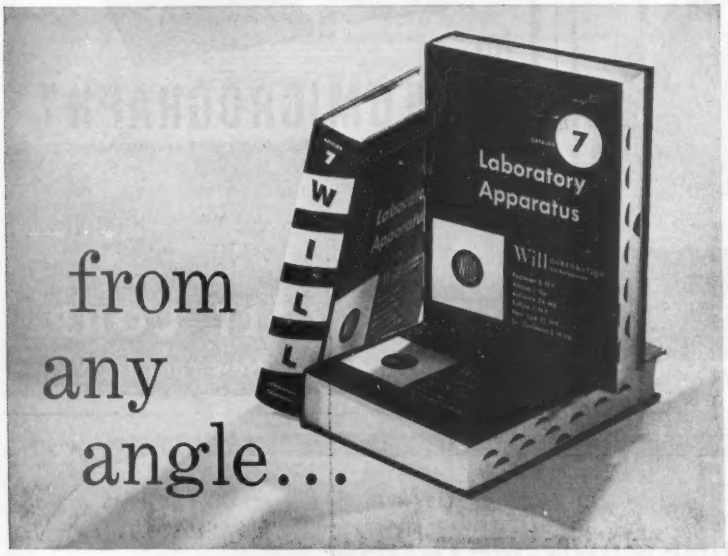
20–22. Pulmonary Circulation Conf., Chicago, Ill. (Wright Adams, Chicago Heart Assoc., 69 W. Washington St., Chicago 2.)

20–23. International Assoc. for Dental Research, annual, Detroit, Mich. (D. Y. Burrill, Univ. of Louisville, School of Dentistry, 129 E. Broadway, Louisville 2, Ky.)

23–26. American Assoc. of Dental Schools, annual, Detroit, Mich. (M. W. McCrea, 42 S. Greene St., Baltimore 1, Md.)

23–29. American Soc. of Photogrammetry, 24th annual, jointly with American Cong. on Surveying and Mapping, 18th annual, Washington, D.C. (C. E. Palmer, ASP, 1515 Massachusetts Ave., NW, Washington 5.)

24–26. Aero Medical Assoc., 29th annual, Washington, D.C. (T. H. Sutherland, Box 26, Marion, Ohio.)



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24–27. Institute of Radio Engineers, natl. conv., New York. (G. W. Bailey, IRE, 1 E. 79 St., New York 21.)

26–28. American Power Conf., 20th annual, Chicago, Ill. (Illinois Inst. of Technology, 35 W. 33 St., Chicago 16.)

27–29. Mechanisms of Hypersensitivity, 8th internatl. symp., Detroit, Mich. (W. J. Nungester, Dept. of Bacteriology, Univ. of Michigan, Ann Arbor.)

27–29. National Science Teachers Assoc., 6th natl., Denver, Colo. (R. H. Carleton, NSTA, 1201 16 St., NW, Washington 6.)

27–29. Optical Soc. of America, annual, Washington, D.C. (K. S. Gibson, National Bureau of Standards, Washington 25.)

29. South Carolina Acad. of Science, annual, Charleston. (Miss M. Hess, Dept. of Biology, Winthrop College, Clemson, S.C.)

29–30. American Psychosomatic Soc., 15th annual, Cincinnati, Ohio. (T. Lidz, 551 Madison Ave., New York 22.)

30–3. American College Personnel Assoc., annual, St. Louis, Mo. (L. Riggs, DePauw Univ., Greencastle, Ind.)

April

1–3. Corrosion Control, 5th annual conf., Norman, Okla. (M. L. Powers, Extension Div., Univ. of Oklahoma, Norman.)

2–4. American Assoc. of Anatomists, annual, Buffalo, N.Y. (L. B. Flexner, Dept. of Anatomy, School of Medicine, Univ. of Pennsylvania, Philadelphia 4.)

2–4. Instruments and Regulators Conf., Newark, Del. (W. E. Vannah, Control Engineering, 330 W. 42 St., New York 36.)

4–5. Southern Soc. for Philosophy and Psychology, annual, Nashville, Tenn. (W. B. Webb, U.S. Naval School of Aviation Medicine, Pensacola, Fla.)

7–11. American Assoc. of Cereal Chemists, annual, Cincinnati, Ohio. (J. W. Pence, Western Utilization Research Laboratories, Albany, Calif.)

8–10. Electronic Waveguides Symp., New York. (J. Fox, Microwave Research Inst., Polytechnic Inst. of Brooklyn, 55 Johnson St., Brooklyn 1, N.Y.)

9–12. National Council of Teachers of Mathematics, Cleveland, Ohio. (M. H. Ahrendt, NCTM, 1201 16 St., NW, Washington 6.)

9–14. Applied Psychology, 13th internatl. cong., Rome, Italy. (L. Meschieri, National Inst. of Psychology, Rome.)

10–11. American Inst. of Chemists, annual, Los Angeles, Calif. (L. Van Doren, AIC, 60 E. 42 St., New York 17.)

10–12. Biometric Soc., ENAR, Gatlinburg, Tenn. (T. W. Horner, General Mills, Inc., 400 Second Ave. South, Minneapolis 1, Minn.)

10–12. National Speleological Soc., annual, Gatlinburg, Tenn. (G. W. Moore, Geology Dept., Yale Univ., New Haven, Conn.)

10–12. Ohio Acad. of Science, annual, Akron, Ohio. (G. W. Burns, Dept. of Botany, Ohio Wesleyan Univ., Delaware, Ohio.)

11. Vitamin B-12 Symp., New York, N.Y. (Miss J. Watson, 451 Clarkson Ave., Brooklyn 3, N.Y.)

11–12. Eastern Psychological Assoc., annual, Philadelphia, Pa. (G. Lane, Dept. of Psychology, University of Delaware, Newark.)

11–18. Horticultural Cong., 15th internatl., Nice, France. (Secretariat General, 84, rue de Grenelle, Paris 7e, France.)

13–14. American Soc. for Artificial Internal Organs, Philadelphia, Pa. (G. Schreiner, Georgetown Univ. Hospital, Washington 7.)

13–18. American Chemical Soc., 133rd, San Francisco, Calif. (R. M. Warren, ACS, 1155 16 St., NW, Washington 6.)

13–19. Federation of American Societies for Experimental Biology, annual, Philadelphia, Pa. (M. O. Lee, FASEB, 9650 Wisconsin Avenue, Bethesda 14, Md.)

14–16. Automatic Techniques Conf., Detroit, Mich. (J. E. Eiselein, RCA, Bldg. 10–7, Camden 2, N.J.)

14–18. American Assoc. of Immunologists, annual, Philadelphia, Pa. (F. S. Cheever, Graduate School of Public Health, Univ. of Pittsburgh, Pittsburgh 13, Pa.)

14–18. American Soc. for Experimental Biology, annual, Philadelphia, Pa. (J. F. A. McManus, Univ. of Alabama Medical Center, Birmingham.)

14–18. American Soc. of Biological Chemists, annual, Philadelphia, Pa. (P. Handler, Dept. of Biochemistry, Duke University School of Medicine, Durham, N.C.)

15–17. Gas Measurement, 34th annual conf., Norman, Okla. (M. L. Powers, Extension Div., Univ. of Oklahoma, Norman.)

16–25. Instruments, Electronics and Automation Conf., London, England. (Industrial Exhibitions Ltd., 9 Argyll St., London, W.1.)

17–19. Association of Southeastern Biologists, annual, Tallahassee, Fla. (J. C. Dickinson, Jr., Dept. of Biology, Univ. of Florida, Gainesville.)

17–19. Eastern Colleges Science Conf., 12th annual, Wilkes-Barre, Pa. (Mrs. E. Stevens, Wilkes College, Wilkes-Barre.)

18. Iowa Acad. of Science, annual, Des Moines. (C. H. Lindahl, Dept. of Mathematics, Iowa State College, Ames.)

18–19. Arkansas Acad. of Science, annual, Little Rock. (L. F. Bailey, Botany Dept., Univ. of Arkansas, Fayetteville.)

19–21. American College of Apothecaries, Los Angeles, Calif. (R. E. Abrams, Hamilton Court, 39th and Chestnut St., Philadelphia, Pa.)

20–22. American Assoc. of Colleges of Pharmacy, annual, Los Angeles, Calif. (G. L. Webster, College of Pharmacy, Univ. of Illinois, 808 S. Wood St., Chicago 12.)

20–23. Chemical Engineering Conf., Canada-United States, Montreal, Quebec. (H. R. L. Streight, DuPont Company of Canada, P.O. Box 660, Montreal.)

21–23. American Oil Chemists' Soc., Memphis, Tenn. (Mrs. L. R. Hawkins, AOCS, 35 E. Wacker Dr., Chicago 1, Ill.)

21–28. American Industrial Hygiene Assoc., annual, Atlantic City, N.J. (G. D. Clayton, George D. Clayton and Associates, 14125 Prevost, Detroit 27, Mich.)

(*See issue of 17 January for comprehensive list*)

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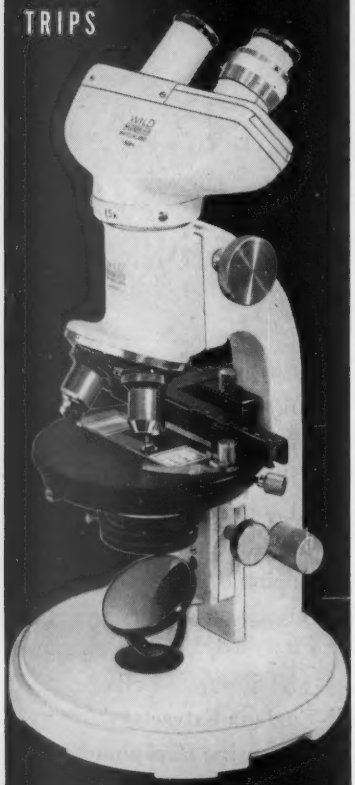

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■ FRACTION COLLECTOR, manufactured by LKB-Produkter of Sweden, takes 240 tubes directly in one turntable disk or in ten groupings of 24 tubes each. The basic assembly can collect, on a preset-time basis, fractions from one to four columns. Addition of related subunits permits both preset-time and preset-volume collection to be made. Individual racks of 24 tubes can be removed as collection is completed; these racks can then be replaced with racks containing empty tubes. The switches are of sealed mercury type. (Ivan Sorvall, Inc., Dept. S848)

■ PORTAL RADIATION MONITOR is designed to fit into a conventional doorway without obstructing traffic. The unit contains separate visual and audible alarms that indicate presence of contamination on the left or right side, head, or feet, of the person passing through the doorway. Gamma and hard beta radiation are detected. Response time is 1 to 2 sec. (Nucleonic Corporation of America, Dept. S849)

■ RATIOMETER is capable of measuring a-c voltage ratios between 10^{-6} and 1.111111 with accuracy of 5 per million at unity ratio or 500 per million at a ratio of 100 to 1. Two models provide an overall frequency range of 40 to 2000 cy/sec. The instrument employs an a-c bridge circuit with a variable transformer in the balance arm. A transistorized amplifier feeds the output signal to the deflection plates of an external oscilloscope for null indication. The effects of quadrature and harmonics are eliminated by quadrature-voltage injection and filtering. (Transformers Inc., Dept. S858)

■ VAN DE GRAAFF ACCELERATOR produces 1.5 Mv of ionizing radiation at a cost of less than $20,000 per kilowatt. Power rating is 2.5 kw, but peak power of 4 to 5 kw is obtainable. The generator's electron beam is scanned through a thin aluminum window located 66 in. below the unit's base plate. (High Voltage Engineering Corp., Dept. S861)

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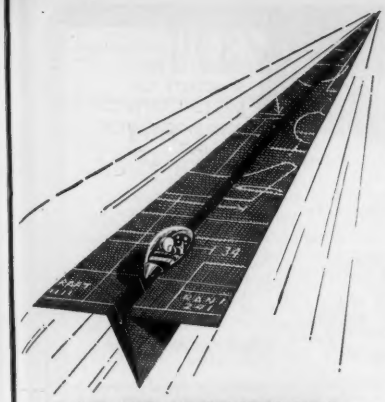

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■ LINEAR FORCE MOTOR is available with strokes to 0.1 in. and force outputs to 2 lb. Accuracy of the relationship between current and force is better than ±1 percent. Standard coil resistances from 200 to 4000 ohm, and resonant frequencies from 10 to 100 cy/sec are available. The motors, measuring 2 in. long and 1⅝ in. in diameter, can be supplied completely sealed. (Mandrel Industries, Inc., Dept. S868)

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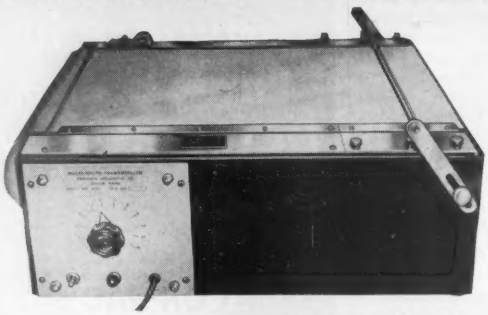

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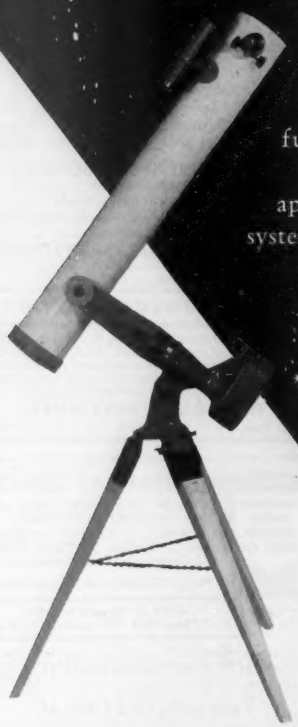

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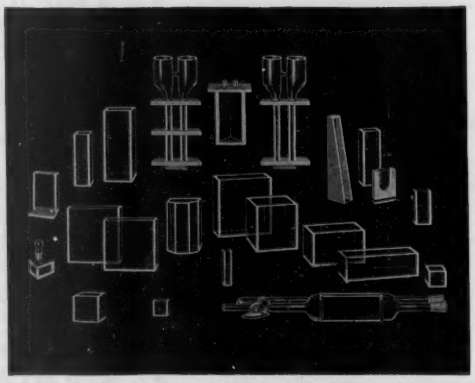

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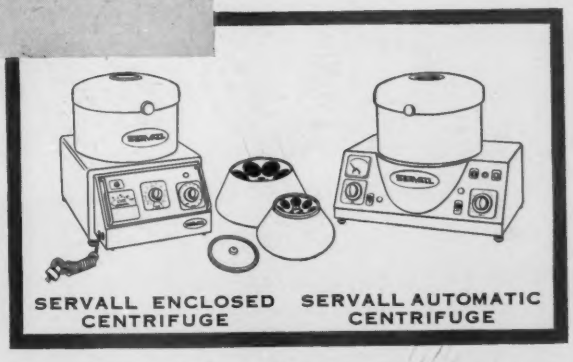

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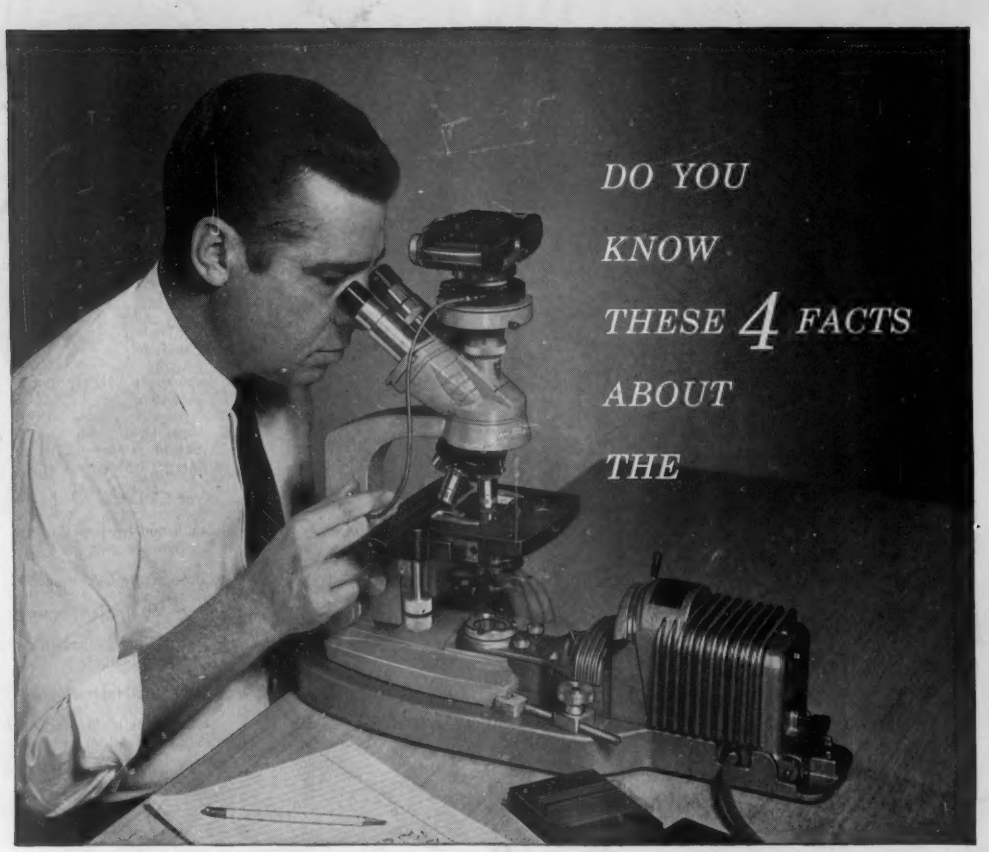

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